Systematic Zoology

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Systematic Zoology

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Principles of Classification as Illustrated by Studies of Arachnida

ALEXANDER PETRUNKEVITCH

It seems almost preposterous that the role of systematics in zoology should be a subject still worthy of discussion in a symposium in the middle of the twentieth century. It is a natural property of the human mind to apply some reasonable system of classification to animate and inanimate objects within its circle of experience. Yet this fundamental attribute of our mind and the simplicity of the logical principles involved in classification are the unwitting cause of the gradual drop in respect for systematics among other biologists.

After a luxurious efflorescence of systematics in the past century the interest of biologists was diverted into experimental fields alluring to the inquisitive mind and holding out the promise of clear answers to moot questions which previously were considered to be unanswerable. The public interest, too, shifted farther and farther away from systematics toward experimental sciences until the prestige of a systematist fell almost to the level of a cataloger of curios. Forty years ago no less an authority in genetics than Davenport ridiculed my point of view in a repartee to my lecture on the relation between the individual, the species, and the environment. He maintained that the *species* is nothing but a figment of imagination, that *individuals* do not exist and that the only entities which do exist and can be dealt with scientifically are the *unit characters*. Many times since that occasion have I heard distinguished biologists point with derision at the work of systematists, which they considered to be not only old-fashioned, out of date, and unscientific, but even a useless pastime or at best an occupation resulting in the preparation of handy check-lists. Nor is such an estimate of the low value of much of the work done in systematics entirely unjust and baseless, for a great deal of it is no more than an unpretentious study of some local collection, or a description of a few new species which have no genetic or geographic relation to each other, and at best a revision of a genus or of a family, a revision lacking a wider background into which to fit the possible conclusion from its study and therefore useless to other zoologists as material for their discussion of what they believe to be the basic problems.

Yet the essence of systematics is not the creation of handy classifications, not the preparation of keys useful for the identification of species, nor the production of check-lists for zoogeographical studies. All this is being done because it has to be done when one is confronted

with the problems of evolution. For *systematics is that branch of biological science which is engaged in unraveling the mystery of evolution by the only method by which this can be done, namely by a comprehensive study of relationships between living beings, based on all available data.* The logical conclusions drawn from an analysis of these data are then used as a guide to the *actual* path which each group of animals has followed in the progress of its evolution, not simply one of the numerous possible paths presented by the phenomena of transmission of genetic characters. Classification is the shorthand of systematics, the latin names used in nomenclature are its symbols. If and when classification serves merely practical purposes, as in the preparation of keys, its shorthand record will give no more than a list of goods, transmitted in code for the sake of brevity, to acquaint the interested party with the stock of available items. But if the classification used by an author expresses his ideas of relationships between animals, based on a comprehensive survey of evidence, and if the reader possesses the prerequisite knowledge to follow the logic of the author's constructive thought, then each generic name and each name of a family and of the higher groups will fall in their proper place and form a complete picture of genetic relationship on the background of geological past.

All this is a truism. I am fully aware of it, and if nevertheless I repeat it here, it is because the value and role of systematics are so intimately connected with the comprehension of its nature. The rapidly growing range of all sciences makes it more and more difficult to encompass the ever widening circle of knowledge necessary for the pursuit of the complicated and intricate problems of systematics. For this reason I shall discuss here the place of systematics in zoology only inasfar as it is concerned with our knowledge of Arachnida, the group of animals most familiar to me.

During the past century the classification of all air-breathing Arachnida was based entirely on external characters. Thus the possession of only four pairs of legs by Tardigrada and Linguatulida was considered even by Leuckart to be sufficient evidence of their arachnid nature, even though their legs are not segmented, and the general appearance of their body has no resemblance to typical representatives of the class Arachnida. No professional arachnologist shares that view at present, because it is well known that the internal organization and the development of both tardigrades and linguatulids have nothing in common with arachnids. A little later in the past century Ray Lankester included the kingcrabs or Xiphosura in the same class with terrestrial Arachnida. He based his ideas of their close relationship not only on the segmentation of their body and the number of their cephalothoracic appendages but on certain similarities of their internal organs as well. His classification prevailed until quite recent investigations and greatly increased knowledge in all fields of biology forced us to separate the Xiphosura from the Arachnida and to assign them to a class of their own. Something similar happened in the case of the Eurypterida and Pycnogonida. Today we agree that all these arthropods are related to each other, but exhibit sufficiently important and fundamental differences in the structure of their internal organs to require their segregation as separate classes in a single subphylum Chelicerata.

The name "Chelicerata" is derived from the name of the appendages which are most characteristic of the four classes but are not found in that particular modification in other arthropods. The name does not, however, do full justice to the reasons involved in the segregation of the four classes in one subphylum. Comparative anatomical, embryological and neurological investigations have shown that the possession of chelicerae is cor-

related with the loss of the second portion of the brain, the so-called deutocerebrum or midbrain, which is present in Crustacea, Myriapoda, and Hexapoda. Neurology has shown that the cheliceral nerves supplied by the tritocerebrum are homologous with the nerves of the second pair of antennae in Crustacea and with the antennae in insects. Therefore the ancestors of all Chelicerata must have possessed a midbrain which they subsequently lost before this ancestral stock split up into four classes because of the development of different trends and modifications of other internal organs. This is a clear example of the difference between classifications based on nothing but external characters with disregard or lack of knowledge of other organs and of their development and ones based on correlation of various characters and on inferences drawn from diverse fields of study and culminating in a summation of available knowledge expressed in the present definition of the subphylum by its antiquated name.

With the restriction of the class Arachnida to air-breathing Chelicerata even when some of them live in water, but breathe air or, lacking a respiratory system of organs, depend on other methods for oxygen intake, their further classification still requires clear correlation of external characters with internal anatomy, if we are to avoid the pitfalls of hasty conclusions based on outward resemblances. In this respect the chief difficulty lies in the evaluation of the importance of changes and of their sequence in time. Arachnologists are in agreement on the status of most orders, the mites being the chief subject of controversy. In their case there is a tendency in some quarters to remove the order Acari from the class Arachnida and to raise it to the rank of an independent class. This procedure could amount to a merely academic discussion as to whether their characters are sufficiently different from those of the other orders to require the restriction of the class Arachnida to the latter. In that case a new superclass would have to be created for the two classes Arachnida and Acari, because of their closer relationship to each other than to the Eurypterida, Xiphosura, and Pycnogonida. The other alternatives would be to retain the Acari in the class Arachnida either as a subclass or as an order equivalent to the other orders. One thing is certain, and that is that a decision in this matter has to be based on correlation of characters. To propose a classification as artificial as that of Oudemans, which was based on the movability and immobility of the coxae, is a glaring example of a lamentable misconception of the principles of systematics, exhibited by a very distinguished scientist. To show the fallacy of his classification it suffices to recall to your mind the fact that the degree of movability of the same pair of coxae may vary considerably in closely allied families and that in numerous Arachnida, such as scorpions and harvestmen, some of the coxae are movable and others immobile in the same individual. In this case we even do not need to search for some correlation between the coxae and the internal organs because the presence of both types of coxae in the same individual is sufficient proof that no such correlation exists.

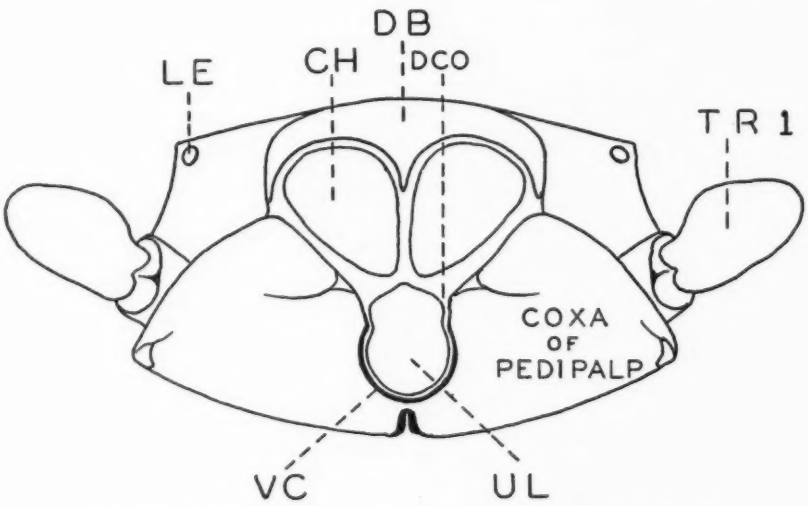
In characterizing the class Arachnida I have been accustomed to give as one of its most important characters the position of the genital opening in both sexes on the segment which corresponds to the second embryonic abdominal segment. In scorpions the first embryonic segment disappears in the course of development so that in the adult the second embryonic segment takes its place and is represented by the first visible tergite and the genital opercula. In Acari segmentation is greatly obscured by various features of their development. Fusions, shifting of abdominal segments and of appendages out of their original position, loss of external segmentation, loss

of two pairs of appendages in the suborder Tetrapodili, all this makes it very difficult to determine with certainty the position of the genital opening with reference to the embryonic segmentation. The difficulty is further increased by the fact that in the parasitic family Demodicidae the genital opening of the female is in its usual position on the ventral surface of the abdomen, but in the male it is situated on the dorsal surface of the capitulum. Evidently this position is the result of a secondary shift from the original one. Some Acari possess salivary glands which are lacking in all Recent Arachnida of the other orders. Some Acari have no heart and no respiratory organs of the type found in other Arachnida, the function of respiration having been taken over by the body wall and the hindgut. The mouthparts, i.e., the two anterior pairs of appendages, are also often so modified in Acari that they lose all resemblance to familiar arachnid chelicerae and pedipalpi. The legs, too, are in many cases reduced to mere stumps with bristles, without a trace of segmentation. Yet all these modifications, however great and confusing, are undoubtedly of secondary origin, the result of adaptation to special requirements not found in other Arachnida. When corresponding internal and external organs are examined in those species of Acari in which they are less modified, the total assemblage of their characters is in harmony with that of other Arachnida and not with any other group of Arthropoda.

The separation of the class Arachnida into orders was originally based on nothing but external characters. Such a classification did not present any difficulty because these characters are distinct, and their correlation with other characters, while either poorly known or not known at all, nevertheless exists in many cases. Thus spiders, at least some of them, were readily recognized as such thousands of years ago because of their ability to weave webs. The order Aranei could be easily separated from all other orders of the class by the single, externally visible character, the presence of spinnerets on the abdomen. This could be sufficient for purposes of identification, but would be woefully inadequate for understanding the complexity of their organization, the advantages and disadvantages of its structure in the combat for existence, and the origin and reason of their unique patterns of behavior. Nor could such a single character, no matter how distinctive and conspicuous it may be, help to establish the nature of a fossil, when its preservation is such that the presence or absence of spinnerets cannot be determined by examination, and identification has to depend upon other characters. The general appearance of the specimen is usually of little help in such a case. There are many instances of wrong identification in the past, at the time when the picture of correlated structures in Arachnida was not yet sufficiently well known. In Recent Arachnida the original classification was retained for a long time because in the majority of their orders the external characters happen to be correlated with certain features of their internal anatomy. The reasons for the necessity to split the old order Pedipalpi is even at present not fully appreciated by many arachnologists. First Pocock and more recently Millot raised the suborders Uropygi and Amblypygi to the rank of independent orders, while I proposed the inclusion of the Schizomidae in an order of their own. The modification of the pedipalpi from organs for chemoreception and tactile usage into raptorial appendages creates the false impression of genetic relationship in preference to independent functional adaptation. But taking every system of organs under consideration, one cannot escape the conclusion that the Amblypygi or Phrynichida, as I proposed to call them, are much closer related to spiders than to whipscorpions. The Schizomida, treated by Pocock and Millot as a suborder of the

order Uropygi, show unquestionably closer relationship to the Thelyphonida because the possession by both of a camarostome (figure 1) places them in the unique position of having in common a singular modification of their mouthparts not found in any other Recent Arachnida. Yet the rest of their anatomy is sufficiently different to require, in my opinion, their separation into independent orders. This separation must have taken place in the distant geological past, because the Thelyphonida, or true whip-scorpions, possessed already in the Carboniferous the same external features as their Recent descendants and presumably had the same internal anatomy as the latter, a circumstance which makes the derivation of the Schizomida from the Theliphonida or *vice versa* impossible.

Extinct orders present difficulties of their own when one is confronted with the problem of their classification. Apart from the fact that even their external anatomy is not yet completely known, practically nothing is known of their internal anatomy. It is, indeed, a very unusual and fortunate circumstance that the outlines of the heart are preserved in one specimen of *Orthotarbus robustus,* a representative of the order Architarbi, from the Pennsylvanian of Mazon Creek, Illinois, described by me in 1945. It is also fortunate, though less remarkable, that book-lungs and pulmonary apodemes are preserved in several specimens of Anthracomarti and Haptopoda, and some muscles in Trigonotarbi. But that is all, and our conclusions concerning the internal anatomy of fossil Arachnida have to be based on the logical probability that the same or a closely similar cor-



CAMAROSTOME OF MASTIGOPROCTUS

FIGURE 1. Transverse section through the anterior region of the cephalothorax of the whip-scorpion *Mastigoproctus giganteus,* common in Arizona and Mexico. CH–chelicera. DB–doublure of carapace. DCO–dorsal opening of the camarostome. This opening is formed by the wall of the upper lip (UL) and that of the coxa of the pedipalp. The camarostome is a curved narrow space between the upper lip and the proximal dorsal wall of the coxa of the pedipalp (VC). The mouth opens into the camarostome and is not visible from the outside. The two coxae forming the camarostome are permanently grown together and possess a common cavity. LE–lateral eye. TR 1–trochanter of first leg.

relation between external and internal structures prevailed in extinct orders as in living representatives of orders which survived. Indeed, such a conclusion is quite reasonable, but it does not have the value of factual evidence. Fortunately, externally visible displacement of the mouth in all Recent Arachnida is directly correlated with changes in the structure of the nervous system and may be seen also in other classes of the subphylum Chelicerata. In all Recent Arachnida the displacement of the mouth reaches only a point in front of the pedipalpal coxae. When, therefore, I noticed that in fossil species of the genus *Anthracomartus* the chelicerae are wedged in between the pedipalpal coxae (figure 2) and that the mouth is situated behind

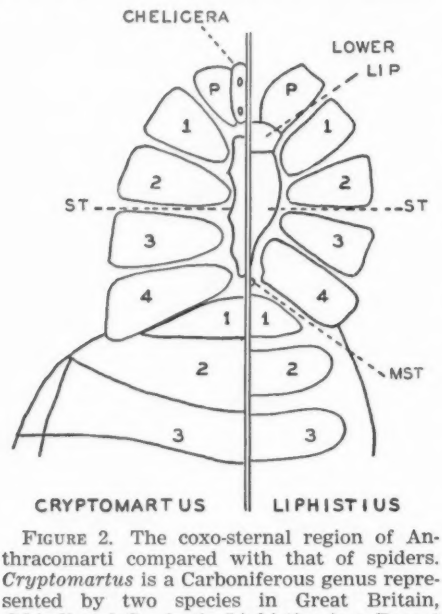


FIGURE 2. The coxo-sternal region of Anthracomarti compared with that of spiders. *Cryptomartus* is a Carboniferous genus represented by two species in Great Britain, *C.hindi* and *C.priesti*. *Liphistius* is a Recent genus of the most primitive family of spiders with a segmented abdomen, two pairs of lungs, five pairs of cardiac ostia, paraxial chelicerae, and four pairs of spinnerets. The genus is represented by several species in Indo-Malaysia. P–the coxa of the pedipalp. ST–sternum. MST–the metasternite reduced to a small tubercle.

the latter, the conclusion became inevitable that these fossils not only differed from Recent Arachnida in the arrangement of their coxo-sternal region but actually presented a further step in their evolution, notwithstanding the fact that they became extinct millions of years ago.

A number of genera were placed in the order Anthracomarti because their dorsal surface showed great resemblance to that of *Anthracomartus*, while the ventral surface was very inadequately known. Now we have learned that all species with an arrangement of the coxae of the same type as in *Anthracomartus* possess on their dorsal surface five rows of tergal plates, while all species with only three rows have an arrangement of coxae closely resembling that of spiders. Thus an externally visible character was found to be correlated with an equally visible character on the opposite surface of the body, but a character involving in Recent Arachnida fundamental embryological changes. Consequently the old order Anthracomarti included genetically incongruous families and had to be split into two orders, Anthracomarti and Trigonotarbi. The correlation between the number of rows of tergal plates and the arrangement of the coxae is nevertheless not a fundamental one, inasfar as the tergal plates are concerned. It is of a later origin than the backward displacement of the mouth. This is evidenced by the fact that the same arrangement of the coxae as in the Anthracomarti, and the same position of the mouth behind the pedipalpal coxae, are characteristic also of the Haptopoda, another extinct order. The only known species of this order, *Plesiosiro madeleyi*, has undivided tergal plates extending as complete single sclerites from the left to the right side of the dorsal surface. Therefore the displacement of the mouth must have preceded the purely external change involved in the subdivision of the tergal plates. The proposed segregation of the

two orders Anthracomarti and Haptopoda in a subclass of their own, the Stethostomata, expresses not only the existence of a closer relationship between them but also their genetic difference from the other orders.

A similar structure of the coxo-sternal region is found not only in the Stethostomata but also in the classes Xiphosura and Eurypterida. These classes are certainly more distantly related to the Stethostomata than the latter are to other Arachnida. It follows *eo ipso* that this arrangement of the coxae is the result of a trend antedating the separation of the class Arachnida from the ancestral stock of the Chelicerata. Were this not so, we would have to postulate that the Arachnida are a class of polyphyletic origin. It seems to be not only simpler but much more reasonable to assume that this old fundamental trend, which is common to all Arthropoda, reached its climax independently in three classes of Chelicerata and that in the class Arachnida it became coupled with some lethal character, so that only such Arachnida survived in which that trend became inoperative. If it were argued that the identity of the arrangement of the coxae in Stethostomata, Eurypterida, and Xiphosura is in itself sufficient evidence of their close genetic relationship, requiring their joint segregation in a superclass, separate and distinct from the Arachnida, then it would be still more difficult to explain the similarities between essential structures in Stethostomata and the other Arachnida on any other basis than converging evolution, without a single advantageous factor in its support and restricted to changes only of internal organs.

One of the most important trends in all Arthropoda is the one involving the loss of abdominal segments. This loss is accomplished either by progressive shortening of a segment, until the corresponding ring of the body wall has completely disappeared, or by progressive peripheral

Chelicerata (subphylum)
- Xiphosura (class)
- Eurypterida
- Pycnogonida
- Arachnida
  - Latigastra (subclass)
    - Scorpiones (order)
    - Pseudoscorpiones
    - Opiliones
    - Architarbi
    - Acari
  - Soluta
    - Trigonotarbi
  - Stethostomata
    - Haptopoda
    - Anthracomarti
  - Caulogastra
    - Latisterna (superorder)
      - Palpigradi (order)
    - Camarostomata
      - Schizomida
      - Thelyphonida
      - Kustarachnae
    - Labellata
      - Phrynichida
      - Araneae
        - Liphistiomorphae (suborder)
        - Mygalomorphae
          - Octostiatae (branch)
          - Sexostiatae
        - Hypochilomorphae
        - Dipneumonomorphae
          - Trionychae
          - Dionychae
          - Quadrostiatae
        - Apneumonomorphae
          - Proterotracheatae
          - Deuterotracheatae
    - Cucullifera (superorder)
    - Rostrata

Table showing the category status of the names of classes, orders, etc. mentioned in the text.

constriction of posterior segments which are thus transformed into a pygidium and finally disappear. When the peripheral constriction affects the first abdominal segment, the latter is transformed into a pedicel. The two methods, shortening and peripheral constriction, are fundamentally different, not only from a mechanical point of view but also in their effect on subsequent changes. Shortening may involve several subsequent segments and, when combined with fusion of adjacent portions of the body wall, may lead to a complete disappearance of abdominal segmentation, as, indeed, happens in Acari. An arachnid with a pedicel can never revert to a condition in which the abdomen is broadly joined to the cephalothorax, for that would require a reversal of the trend, a progressive expansion of the pedicel or of the anterior wall of the second segment. The probability of such a reversal being infinitesimal, it may be accepted as certain that neither of the two types of abdominal connection was produced from the other subsequent to the loss of the first segment through its shortening or to its transformation into a pedicel through peripheral constriction. Following this reasoning we arrive at the division of the class Arachnida into four subclasses proposed by me, Latigastra, Caulogastra, Soluta, and Stethostomata. The subclass Soluta, which includes only the fossil order Trigonotarbi, is not sufficiently well known to make its position in the system as clear and natural as in the case of the other three subclasses.

To trace the relationship between the orders within the limits of each subclass is a difficult and conjectural matter in the present state of our knowledge. In the case of the Latigastra I feel certain only of the existence of close relationship between the extinct order Architarbi and the suborder Cyphophthalmi of the order Opiliones. The Architarbi do not exhibit much resemblance to other Opiliones, whether fossil or living ones. It may be that the suborder Cyphophthalmi will have to be raised to the rank of an independent order, but that will have to be decided by future investigations of the comparative anatomy of all three suborders of Recent Opiliones. In the case of the Caulogastra a definite relationship exists between the Thelyphonida, Schizomida, and Kustarachnae, as evidenced by the structure of their mouthparts. Some relationship, though less well marked, exists between the Aranei and the Phrynichida. The available fossil material is quite insufficient to be of any help in the study of relationships between the orders of Arachnida.

Having obtained a glimpse into the *macroevolution* of Arachnida and expressed the results of our analysis in the usual shorthand of systematics, let us now proceed with an examination of the later steps of evolution within the orders. For this purpose I shall select only two orders because we know so little of the corresponding steps within the other orders. The first order which I wish to consider is the scorpions. Even at this moment their classification, as used by specialists, remains as unrelated to their comparative internal anatomy, as it was a hundred years ago. To be sure, it has been changed more than once since that time, but the changes consisted only in the selection of some other set of external characters, without a single attempt at correlation. I am sorry to admit that I myself had no time to make comparative anatomical studies of Recent scorpions. However, the disappearance of the first abdominal segment, discovered and described many years ago, was confirmed later and independently of each other by Buxton and myself. It was therefore natural to expect that the first abdominal segment may still be present in the oldest fossil representatives of the order. Reexamination of the holotype of *Proscorpius osborni*, the only existing American specimen of Silurian scorpions, convinced me that the first tergite is still visible in

outline under the carapace. In the Pennsylvanian *Mazonia woodiana* the first tergite is fully exposed although that American fossil is of a much later geological period. Thorell's description of the European Silurian *Palaeophonus nuncius* convinced me that this scorpion also shows the outlines of the first tergite under the carapace. So I proposed to divide the order Scorpiones into two suborders, Protoscorpiones and Euscorpiones, on the basis of the presence or absence in the adult individuals of the first abdominal embryonic segment, i.e., on the basis of a fundamental character. Therein my subdivision differs essentially from that of Pocock, proposed by him in 1911 and based on a purely external character, the bilobed shape of the sternal plates in scorpions of his suborder Lobosterni and their straight-edged shape in the suborder Orthosterni. Examination of the European types confirmed the correctness of my point of view, while the bilobed edge of the sternites proved to have only generic value.

The separation of the two suborders into families followed different lines. In the Protoscorpiones the changes seem to have had more to do with the proportions of the legs and the structure of the tarsus than with anything else, and were possibly correlated with a change in habitat. In the Euscorpiones the most important changes involved modifications of the coxo-sternal region. These consisted in a forward displacement of the second and third embryonic sternites until the third and fourth pair of coxae came to abut against the genital opercula (figures 3 and 4). The corresponding tergites were not affected by this displacement. Five Paleozoic families may be readily recognized by the arrangement of their coxae. One of these, the Eoscorpionidae, has the same arrangement as all Recent families and may therefore be regarded as the ancestral stock of the latter (figure 5), because the differences between them are of minor importance by comparison with the changes involved in the forward displacement of the sternites. Since students of Recent scorpions recognize six families, all of which present the same arrangement of coxae, it follows that these Recent families are of a lower grade than the Paleozoic ones in an evolutionary sense. Their separation is based on minor characters, such as the shape of the sternum, the number of tarsal spurs and of lateral eyes, etc., characters not yet correlated with any internal modifications. Scorpions completed their macroevolution in the Paleozoic. Although the number of species must have increased considerably since that time, nothing comparable to these

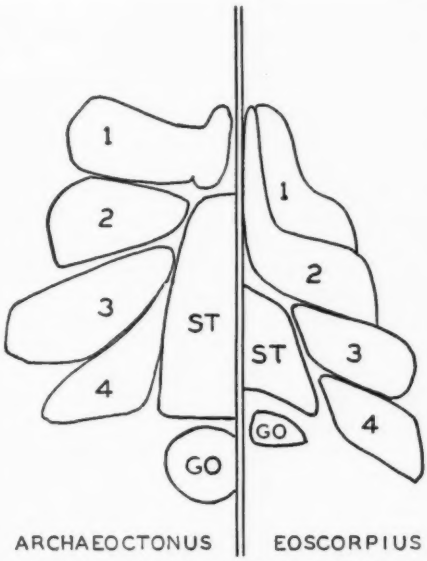


FIGURE 3. Fundamental changes in the coxo-sternal region of scorpions. *Archaeoctonus* is a Carboniferous scorpion and represents the first step in these changes. The sternite of the first pedal segment of the body has been resorbed and its place taken up by the newly developed maxillary lobes of the first coxae. *Eoscorpius* is also a Carboniferous genus but represents the second step in fundamental changes. The sternite of the second pedal segment has also been resorbed and its place occupied by the newly developed maxillary lobes of the second coxae.

changes happened to scorpions in all the millions of years which separate us from the Paleozoic.

It may be interesting in this connection to mention that the only order of Arachnida which has undergone no important changes of any kind since its appearance in the Paleozoic is the order Thelyphonida, or whip-scorpions. Zoologists have often expressed the opinion that the whip-scorpions are more primitive than the scorpions, one of the points in favor of this assumption being the fact that their segmentation is less modified. In my opinion the fact that the Thelyphonida underwent only minor changes in their external anatomy suggests strongly that their internal anatomy remained also practically unchanged. The camarostome which is characteristic of Thelyphonida and Schizomida is a highly specialized modification of the mouthparts (figure 2). It seems to have been in existence already in the Carboniferous species, if one can rely upon the identity of external features and upon the fact that in the Kustarachnae the maxillae are fused even without a trace of a division line. All this speaks rather in favor of the assumption that the whip-scorpions reached the end of their macroevolution at an even earlier geological era. Otherwise it is impossible

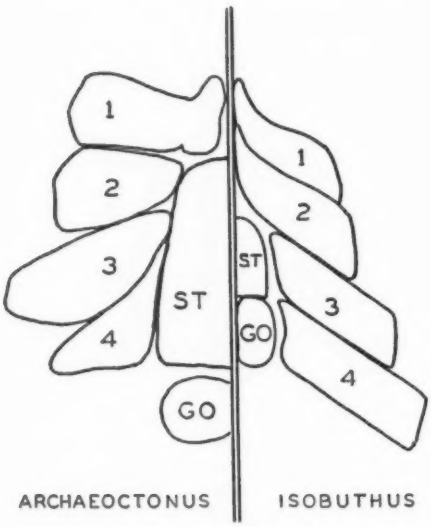


FIGURE 4. The coxo-sternal region of *Isobuthus* compared with that of *Archaeoctonus*. *Isobuthus* is a Carboniferous scorpion and represents the third step in fundamental changes. The sternite of the third pedal segment has been resorbed and into its place moved the fourth sternite, while the genital opercula (GO) occupy the place vacated by the fourth sternite. This proved to be genetically tied to something that caused its extinction.

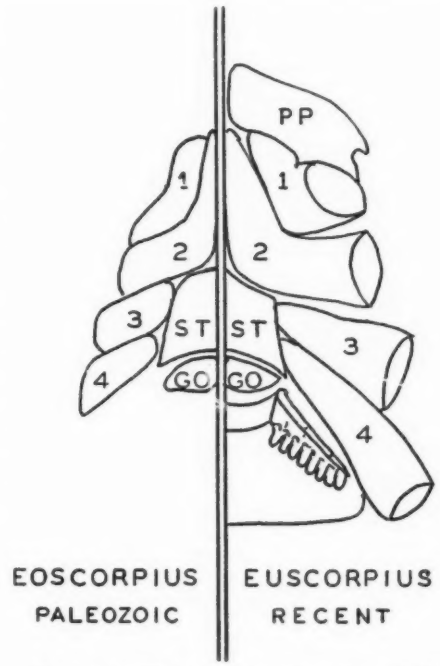


FIGURE 5. The coxo-sternal region of the common Recent Italian scorpion *Euscorpius italicus* compared with the Carboniferous *Eoscorpius* discussed in the explanation to figure 3. It will be seen that there is no fundamental difference between the two. This disposition of coxae, representing the second step in the evolution of families in scorpions proved to be the only viable one. In the formation of Recent families only minor changes have taken place, involving relative proportions and shape but no loss and no displacement.

to understand why, if they are primitive, they alone among all Arachnida did not change in any important feature in the course of two hundred and fifty million years.

Let us now consider the classification of spiders as an expression of their evolution. This subject is particularly dear to me because my attempt to find such a classification on the basis of various correlations and an analysis of their probable sequence met with vigorous objections on the ground that it separates closely related families, brings together unrelated ones, disregards external resemblances, especially those of the palpi, in favor of internal ones, and makes it necessary to assume the existence of converging evolution in a great number of cases. But before taking up these objections and exposing their fallacy, we must briefly review some of the important changes in the principles applied to the classification of spiders by others antedating my own attempts, so as to emphasize the logic of the latter in the light of our present knowledge and thus to provide the ground for its defense and verification.

It is not surprising that the first attempts based on more than merely striking external characters should have been directed toward a correlation of habits with structure. Perhaps the clearest exposition of this principle is found in Walckenaer's first volume of his treatise "Histoire naturelle des Insectes Aptères," published in 1837. He says on page 137, "La ressemblance dans les toiles tissues par les Aranéides, est donc un indice des rapports d'affinités qui existent dans leur organisation. Les Aranéides, qui font des toiles peu différentes entre elles, doivent appartenir au même genre, ou à des genres très-voisins. On est donc certain de former une classification naturelle en groupant les Aranéides d'après les formes de leur toiles." ("The resemblance between webs made by spiders is therefore indicative of affinities existing in their structure." The author refers here to the structure of the spinning organs which he mentions in the preceding paragraph. "Spiders which make webs differing only a little, must belong to the same genus or to closely related genera. One is therefore certain to arrive at a natural classification by grouping spiders in accordance with the types of their webs.") The separation of spiders into sedentary and vagabond families, the subdivision of the former into orb-weavers, sheet-weavers, etc., and of the vagabonds into running, jumping, and laterigrade families persisted almost to the end of the nineteenth century. Thus, even in McCook's third volume of his celebrated "American Spiders and Their Spinning Work," published in 1894, *Uloborus* and *Hyptiotes* are still placed in the same family with *Epeira, Argiope, Nephila,* etc. because of the similarity of their webs. It was only after the publication of Simon's great treatise that *Uloborus* and *Hyptiotes* were finally removed from the family of orb-weavers into a section of cribellated spiders. That section is still recognized by the majority of arachnologists notwithstanding the great diversity of webs made by representatives of different families belonging to it. The cribellum, from which the name of the section is derived, is correlated with a special type of silk glands and with the presence of a calamistrum, i.e., a row or brush of modified bristles on the metatarsi of the fourth pair of legs, features wanting in all other spiders, of which Simon's section Ecribellata is composed. Being a striking external character convenient for purposes of classification, the presence of a cribellum and calamistrum takes precedence in the opinion of the majority of arachnologists over the equally striking, but outwardly invisible dissimilarity of the structure of internal organs in various spiders possessing a cribellum and calamistrum.

Although the internal anatomy of spiders has been frequently studied in the past, such investigations did not concern themselves with a comparison of internal organs in various species until much later. Bertkau was the first to attempt the use

of internal anatomy for purposes of classification in the eighties of the past century. The reason for the unpopularity and failure of his attempt lies in the paucity of our knowledge at that time. Moreover, there was a common misconception concerning functional adaptations, leading to the assumption that they belong invariably to the class of analogies, rather than homologies. Consequently they were supposed to be misleading when used in evidence of relationship. Possibly for the same reason Simon, with whose cooperation work on the comparative anatomy of spiders was done in Paris by Causard, Lamy, and Gaubert, nevertheless built his own remarkable and justly celebrated classification entirely on external morphological characters. Internal anatomy has no place in it. Only in a few instances did Simon adduce arguments in favor of relationship that were based on similarities of certain habits, and then only in cases in which external characters were insufficient for a clear verdict, because of their intergrading features obliterating the limits of separation. Thus, he referred the genus *Roicinus* to the family Lycosidae because of the habit of the female of carrying the egg cocoon attached to her spinnerets, in disregard of the fact that the arrangement of the eyes resembles more that of some Agalenidae and not that characteristic of all Lycosidae. On the other hand, as mentioned above, Simon removed the Uloboridae from the family of orb-weavers on account of their spinnerets. Yet he included in that family for strictly morphological reasons numerous spiders which either build a web of a quite different type or else make no web whatsoever. Simon's first-hand knowledge of species of spiders from all over the world was so tremendous that he was able to notice similarities of external structures which would have escaped observation under other circumstances. But while he thus produced the most comprehensive classification of spiders ever published, he failed to give valid reasons for his assumption that relative simplicity of certain external structures is in itself an evidence of their primitiveness. He seems to have been unaware of the innumerable cases in other invertebrates in which a simplicity of structure is unquestionably the result of a loss of original complexity, an adaptation to some special conditions, an evidence of regressive evolution and not of primitiveness. Only in the case of the Tetragnathinae he chose to disregard the simplicity of their external female genitalia as evidence of primitiveness, because of the high complexity of their male palpi.

The value of Simon's classification rests on the fact that many external characters happen to be correlated with other characters, as we now know, and presumably are so in the majority of cases. However, when I began to study the comparative anatomy of internal organs, the diversity of structure in species referred by Simon to the same family became evident in many instances. The maximum number of cardiac ostia is five pairs, in *Liphistius*. The minimum number is two pairs, with all intermediate numbers found in various spiders. The difference in the number is due not to an increase in the number of ostia from two to five pairs, but to a progressive loss beginning with the last pair, until only the anterior two pairs are left. It also became evident that the loss of book-lungs and their substitution by tracheal tubes is correlated with the loss of cardiac ostia in arachnomorph spiders. At the same time the development of the tracheal system exhibited two different trends. Either it remained simple and restricted to the abdomen, or it produced numerous branches which entered the cephalothorax and not only penetrated its muscular and nervous system but also all appendages to their end. In the case of such a complex tracheal system there is also a correlation between the production of branches and a distinctly advanced position of the tracheal spiracle, which in many cases becomes paired but in other cases remains single. On the basis of these correlations I proposed to divide

all spiders into five suborders, placing at the bottom of their evolutionary scale the Liphistiomorphae, because not only do the twelve abdominal segments persist even in their adults, not only do they possess four pairs of spinnerets of the most primitive type, still showing their origin from two pairs of pleopods, but they also retain the maximum number of cardiac ostia and of book-lungs. At the top of the evolutionary scale I placed the Apneumonomorphae, because they lost all but the anterior two pairs of cardiac ostia and both pairs of lungs, which became replaced by tracheal tubes.

I do not need to dwell here on the details of my classification, because many years have passed since its publication in 1933, and it is sufficiently well known. Notwithstanding the appearance of later classifications, the one proposed by me still remains the only one completely based on a probable sequence of structural changes of internal organs with the assumption of the irreversibility of such changes and of the improbability of their independent and repeated origin as functional adaptations in so many instances. The result is, of course, not a simple reshuffling of Simon's families, but a regrouping of families along totally different principles and the splitting up of some of his families into two or more widely separated ones. Thus his section Cribellata had to be abolished, the family Hypochilidae was raised to a suborder, while the other families were scattered among non-cribellated families of the suborder Dipneumonomorpha (figure 6) and placed there in the vicinity of those families to which they showed the greatest resemblance in other respects. A somewhat similar treatment had to be meted out to Simon's subsection Haplogynae, the chief distinction of which according to Simon's own statement consists in a simplicity of their external sexual organs. The family Clubionidae in his subsection Entelegynae, which also had to be abolished, had to be split up into five families, one of which, the Anyphaenidae, has the minimum number of cardiac ostia and a greatly advanced and complicated tracheal system. For the same reason the Hahniinae and the Argyroneteae, placed by Simon in his family Agelenidae, had to be removed from it and raised to the rank of independent families. With several other families exhibiting the same

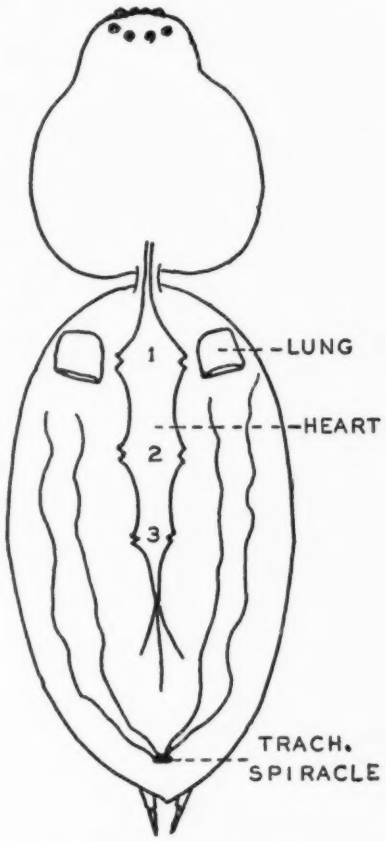


FIGURE 6. The heart and respiratory system of *Agelena*, a genus belonging to the six-ostiate branch Trionycha of the suborder Dipneumonomorpha. The heart has three pairs of ostia. In front is a pair of book-lungs. Close to the spinnerets is a tracheal spiracle which leads into two pairs of simple tracheal tubes restricted to the abdomen.

character, they were placed by me in the division Quadrostiatae of the suborder Dipneumonomorphae (figure 7).

Further subdivision of the suborders is based in my classification on less important changes, whether of a progressive or a regressive nature, but in all cases presumably of later origin. Because of the correlation between internal and external characters, the classification proposed by me does not affect in any way the use of artificial keys and has only theoretical value. The objections to it are, therefore, not concerned with the question of its impracticability but solely with the relative value of the characters selected by me and with the sequence of their appearance or disappearance in the course of evolution, and naturally, as a consequence of this with the relationship and grouping of the families, as proposed by me. For example, one may question my conclusion that the book-lungs were first replaced by tracheal tubes before the heart lost a pair of ostia, assuming instead that the sequence was the reverse of this. If my assumption is correct, then the erection of the suborder *Apneumonomorpha* (figure 8) is justified. If on the other hand the reverse alternative is correct, then such families as Caponiidae and Symphytognathidae should be placed with other quadrostiate families which still possess a pair of book-lungs, in the branch Quadrostiata of the suborder Dipneumonomorpha, and that branch should be elevated to the rank of a suborder. But even in that case or in the case of the assumption that the reduction in the number of cardiac ostia is coupled with the replacement of the book-lungs by tracheal tubes so that both changes take place simultaneously, the logic for the segregation of such families in one group would not be invalidated, but the question would be merely raised as to why in that case the change involved both characters only in two families, while in the other families only one character was affected.

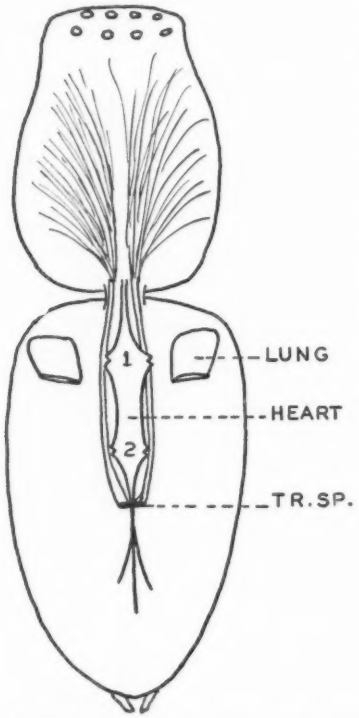


FIGURE 7. The heart and respiratory system of *Anyphaena*, a genus formerly placed in the six-ostiate branch Dionycha but removed by me into the branch Quadrostiata. The heart has only two pairs of ostia. The book-lungs are as in *Agelena*, but the tracheal spiracle is situated far from the spinnerets and leads into a pair of tracheal trunks which enter the cephalothorax and split up into a number of fine tubes, several of which penetrate into the appendages.

But in most of the published criticisms and objections to my classification, the changes in the structure of internal organs are simply left out of consideration. They are disregarded as less important than the visible similarities and differences of external characters. Indeed, there are many cases in which the external characters show strong resemblance to each other in spiders with widely differing internal anatomy, as for example in Cubionidae and Anyphaenidae which

are so much alike that they can be distinguished only by the position of their tracheal spiracle. In other cases the resemblance is less striking, but the differences can be explained without reference to internal organs. In cases in which there is practically no resemblance whatsoever to any other family, as in the case of Senoculidae and Ammoxenidae, the families are placed somewhere within a larger superfamily, like orphans of unknown parentage. The structures generally used in classifications which disregard the internal organs are the mouthparts, the spinnerets, the sternum, the legs and the genitalia, particularly those of the male sex. Thus Bristowe's Group V, Dysderoidea, in disregard of important differences in internal anatomy, includes four families, Dysderidae, Oonopidae, Hadrotarsidae, and Caponiidae. Only the last of these has no book-lungs, but only tracheal tubes, and shares this character not with the other three families of Group V, but with the Symphytognathidae which are placed by Bristowe in his Group VIII, Argiopoidea, in which all the other families have not only a pair of book-lungs, but also three pairs of cardiac ostia, while the Symphytognathidae have only two pairs. The logical and unavoidable consequence of Bristowe's grouping is that the two cases of complex modifications of the respiratory and circulatory systems in Caponiidae and Symphytognathidae represent two identical processes which took place independently in two widely separated groups of spiders, groups differing only by some features of their external characters. If that is not a tacit admission of converging evolution of a most complicated and unusual type, involving only internal organs, then what is it? And what produced them? For if these changes of internal organs are of such little importance that spiders in which they took place may nevertheless be regarded as close relatives of others in which no such changes have taken place, then what kind of physiological correlation has necessitated their independent appearance in totally unrelated families?

Caporiacco's classification takes cognizance of the comparative internal anatomy and follows some of my conclusions, yet still shows the reluctance of that author to apply the same logic to all cases.

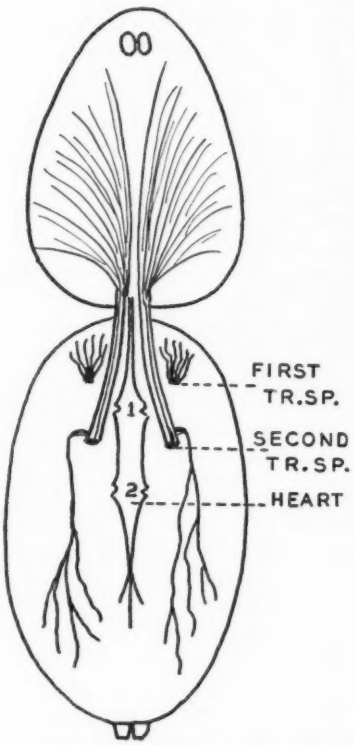


FIGURE 8. The heart and respiratory system of *Caponia*, a representative of the suborder Apneumonomorpha. The heart has only two pairs of ostia. The book-lungs are replaced by the first pair of tracheal spiracles with its short tubes supplying air to the anterior portion of the abdomen. The second pair of spiracles situated not far behind the first pair leads into two pairs of tracheal trunks which enter the cephalothorax and split here into eleven tubes each, some of them extending into the appendages. At the base of the two outer tracheal trunks small tracheal tubes are given off and supply air to the main portion of the abdomen.

Thus, he recognizes the necessity of separating the Apneumones from the other non-cribellated families. He also removes the Hypochilidae from the rest of the cribellated spiders into a group of their own, because they alone possess two pairs of lungs and four pairs of cardiac ostia among all cribellated spiders. Yet he omits my branch Quadrostiata and scatters the families united in it in my classification, among spiders with three pairs of ostia by giving preference to external resemblances. This shows clearly and unequivocally that he accepts an independent origin of the reduction in the number of ostia in each of the nine families. This may be not an impossible phenomenon, but one which certainly begs explanation of the nature of its contributing causes.

The latest attempt made by Archer to base the classification of spiders on the structure of the male palp deserves discussion in this connection. Archer is quite right when he says that "on the whole spider genitalia (including the female genitalia) furnish a valuable basis for discerning taxonomic relationship" (1948, p. 8). Nobody has ever denied this. The question is, *what order of relationship* may be thus discerned? The fundamental structures of the copulatory apparatus of the male, situated always on the last segment of the palpi, are the same in all spiders. They consist of: (1) an elastic membrane which becomes stretched under the internal pressure of the blood preceding and during copulation, thus causing the embolus to protrude; (2) the embolus which serves as organ of copulation; and (3) the blind sac inside the base of the embolus and opening to the outside at the tip of the latter. This blind sac, whatever its specific shape may be, always serves as a temporary reservoir for sperm and must be filled by a special process of pumping, before copulation can take place. All other chitinous parts of the male copulatory apparatus are subsidiary in their function and are nothing but solid sclerites of various shape. Although they received various names and are of great usefulness in identification of species, their function remains completely unknown. The old idea that we have in the genitalia of spiders a perfect example of a lock-and-key adaptation is entirely discredited by careful study. The embolus *does not fit* the corresponding receptacle of the female as a key fits a lock. When a portion of the male copulatory apparatus is broken off and temporarily retained in the female receptacle, an occurrence which has been observed and described many times, but which is not a necessary or an unavoidable consequence of copulation, the cause of the accidental break is not due to a too close fitting between the parts. Neither do the subsidiary structures of the male copulatory apparatus fit any female parts, nor does the female possess sufficient acuity of vision to discern their shape. Presumably they are nothing but organs for tactile excitation of the female. One must also remember that both the fundamental and the subsidiary structures of the male copulatory apparatus appear on the outside of the palpi only after the shedding of the last exuvium, coincident with sexual maturity.

The so-called epigynum of the female, used in identification of species as a valuable character, is the external portion of her copulatory apparatus. It is wanting in many spiders and for this reason cannot be regarded as a fundamental character. Its function is not adequately known. The fundamental structures of the female genitalia, present in all spiders, are a pair of internally situated sperm receptacles. They may be simple blind sacs opening to the outside in front of the genital opening of the uterus, which serves only for the passage of eggs, but not for copulation. In many cases the sperm receptacles are connected with the uterus by special fertilization ducts. Usually there are between the receptacles and the fertilization ducts one or two

pairs of special chambers of as yet unknown function. But whether the epigynum is simple or complex, it is present in its definitive form only in the mature female. It is a sad reflection on our own ignorance that we have to depend for the recognition of the species on the structure of external genitalia, and that even then identification is not always simple and requires the aid of magnification. The spiders, whose eyes are optically not nearly as good as ours, recognize not only their own species but also the sex without difficulty, in some species even before maturity, as for example in *Nephila clavipes* and *Metepeira labyrinthea*. Whatever the distinctive characters may be, which permit spiders to recognize each other, and whatever senses are involved in the process of recognition, the fact itself is sufficient proof of the existence of characters other than those employed by arachnologists and as yet unknown to us. Yet some arachnologists proposed new genera on the single basis of differences in the structure of the male copulatory apparatus and denied the feasibility of identifying certain species when female specimens only are available. The statement that the structure of the male palp shows great similarities in related species, some resemblance in the general plan of structure in related genera and even in some families, is quite true. But it is nevertheless not feasible to accept the proposition that these unisexual structures of adult individuals should have genetic precedence over changes in the structure of internal organs, changes which reach their completion already in the first instar. Yet merely on the basis of such differences in the structure of these unisexual external organs Archer retains the apneumone Telemidae and Symphytognathidae in the dipneumone superfamily Argiopoidea. He also places into a single superfamily Dysderoidea the Loxoscelidae and Scytodidae, both families with a pair of book-lungs, with tracheal tubes limited to the abdomen, and with three pairs of cardiac ostia, with the Caponiidae which have no book-lungs and only two pairs of ostia, and with the Segestriidae, Dysderidae, and Oonopidae, all of which have a pair of book-lungs, tracheal tubes which are profusely branching in the cephalothorax, and two pairs of ostia, merely because their "genital bulb of the male palpus [is] very reduced, subapical and weakly defined into divisions."

In criticizing my classification in a footnote on page 8 of his paper, Archer says that it requires "a series of coincidences in convergent development that are too numerous to be plausible." A little reflection will show anybody beyond all possibility of contradiction that the number of "coincidences" is *exactly the same* whether one accepts my classification or any other classification, because each of these classifications involves the same species. The difference is not in the number of "coincidences in convergent development," but in the value one puts on the organs which show such convergence. In classifications which disregard striking resemblances in the structure of internal organs in favor of similarities in external organs, the former beg explanation, while in the case of the classification proposed by me and still considered by me to be the more plausible one, an explanation is required for the origin of external resemblances. Such resemblances are of common occurrence in various groups of animals, and spiders are no exception to the rule.

The distinguished British arachnologist Bristowe suggested in a light vein during an amicable conversation at our meeting in London that since I have already changed three times my original classification proposed in 1923, I should change my latest effort and return to the first one, which was based on the correlation between external characters and certain habits and which received at the time of its publication much wider recognition than my later attempt to arrange

the suborders, branches, and families in accordance with the problematic evolutionary sequence of changes of internal organs. I can answer his challenge only by a similar friendly challenge, for Bristowe also changed the classification which he used at first, in favor of a new one proposed by him later. Only I would like to make the stipulation that we come first to some agreement on the genetic and evolutionary value of characters in order to determine which of them should be regarded as having had precedence in the sequence of their appearance and therefore should be considered entitled to a higher rank in classification, because in the correct answer to this question is contained the difference between natural and artificial grouping.

Gerhardt and Kaestner, the coauthors of the chapter on spiders in the third volume of Kükenthal's Handbuch der Zoologie, published in 1938, accept the division of arachnomorph spiders into Cribellatae and Ecribellatae and of the latter into Haplogynae and Entelogynae. Since I have already discussed the weakness of this division, I do not need to repeat my arguments here. They also reject my branch Quadrostiata as "unnatural" ("die Unnatur der Abteilung") and intersperse the quadrostiate families between various sixostiate ones. Although they give an excellent account of the general structure and habits of spiders, they evidently consider similarities in the structure of internal organs less important than such of external organs. One of their arguments against my classification is the relatively small number of species studied by me, which in their opinion is inadequate for final conclusions. My studies were limited to carefully selected species, as far as I was able to get them. Compared with the great number of known species of spiders, the number of those which I studied is indeed small. But they are representative of the groups into which Simon placed them and for this reason were selected by me. Whoever doubts that conclusions may be drawn from their study should himself add to our knowledge of the comparative anatomy of spiders by studying as many other species as he may have time and desire to do. Meanwhile the value of any theory may be demontrated by subjecting it to a test. In the case of my classification the test should consist in an examination of other representatives of the Dipneumonomorpha and Apneumonomorpha. I venture to predict that all genera placed by me in these suborders, except possibly such as I have not personally examined but placed on the strength of printed descriptions, will prove to conform with the distinctive characters as specified by me. On my part I am pleased to be in the position to state that I have completed the study of *Glenognatha* and *Mimognatha* which I suspected as belonging to the branch Quadrostiata and not to the sixostiate Trionycha. My expectation was fully confirmed. Both genera have only two pairs of ostia and their tracheal tubes ramify profusely in the cephalothorax. The account of this investigation, accompanied by figures, will be published in the near future. If the correlation of characters, discovered by me and used as a basis for my classification, did not exist, and if these characters could originate independently in unrelated spiders, no prediction could be possible.

I realize, of course, that my deductions and my classification embodying them may be wrong in many details and possibly even in some more important features. Such defects could and should be corrected by further investigations. Years ago I have said that *taxonomy is the mirror of evolution* (1923, p. 145). Being a reflection of one's thoughts, any classification may be imperfect in many respects. My classification assumes the occurrence of convergent evolution of external characters in spiders as a common feature. We are familiar with spiders mimicking ants. We know numerous cases of pro-

tective coloration, of resemblance to lichen and to dry twigs, etc. In all such cases the internal anatomy seems to remain unaffected. Why unrelated spiders resemble each other, we do not know. Nor do we know why their internal anatomy should be so strikingly different. It is a problem which should be thoroughly investigated, without prejudice to any theory. But I submit in all humbleness, yet with a feeling of deep conviction that my classification strikes at the roots of the problem and thus brings nearer to us the value of systematics as the simplest expression of the process of evolution.

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Department of Utter Confusion

"There was issued in 1943 Bull. 64 on the flies; it is dated 1942 because the manuscript was submitted at that time and that is the date that obtains in questions of priority."

For and Against Uniform Endings in Zoological Nomenclature

G. G. SIMPSON

In a recent note (Science, vol. 112, p. 94, 1950) H. B. Stenzel has proposed a series of nine uniform endings for names of taxonomic categories from superorder to subtribe. Several other such systems have been proposed, some of them with over forty endings for different categories. Professor Stenzel has ably presented arguments in favor of his proposal. In brief they are that uniform endings would be more logical and consistent, more stable, simpler, and easier to remember (especially for students). As he also states, final action should be collective and cannot be taken by any one systematist. Even during preliminary discussion it is, however, desirable to present different individual opinions on such proposals.

Professor Stenzel's proposal refers exclusively to the use of uniform endings. As he has emphasized in personal communication, he did not mean to recommend forming names of higher categories (above superfamily) by adding uniform endings to the root of the name of an included genus, although the names in his examples are formed in that way. In fact, at least four different issues may arise:

(1) Use of uniform endings for names of supergeneric categories.
(2) Forming such names from the stems of generic names.
(3) Extension of the type system to supergeneric categories.
(4) Application of the rules of priority to names of such categories.

Under the International Rules of Zoological Nomenclature, strict adherence to which I recommend (with occasional misgivings), (1), (2), and (3) are prescribed for subfamily and family names, which under Article 4 must be formed by adding *-inae* and *-idae*, respectively, to the stem of the name of the type genus. Article 5, which provides that family and subfamily names must be changed when names of their type genera are changed, could be read as implying that rules of priority do not apply to family and subfamily names, but the rule is ambiguous. With acceptance of the Rules, further proposals then refer, except possibly for (4), only to categories above superfamily and to any inserted between genus and subfamily, usually tribes (also sometimes super- and subtribes).

Even though (1), (2), (3), and (4), as above, are logically separable, and even though Stenzel and others divorce proposals for one of them from any of the others, they are related. Any proposal regarding (1) may set up a chain reaction through (2) and (3) to (4). As far as now established by rule or custom, names of supergeneric categories with uniform endings always are based on generic names. In personal communication, Professor Stenzel offers "Primatida" or "Primida" in place of, say, "Hominida" for the order now universally called Primates, but I venture to predict that if uniform endings were used for orders, compounding from generic names would be usual and might become a rule. Sequence from (1) to (2) has almost automatically followed in the few cases when uniform endings have come into use for

orders in some particular class. The now common system for avian ordinal names ending in *-iformes* is an example. All are compounded from generic names.

When names are compounded from generic names, it is almost inevitable that (3) will follow: the name will be taken to imply that the genus is in some sense the type of the higher category to which the generic name extends. In turn, the concept of types is so closely linked to that of priority that (4), application in some way of priority to names of any groups with designated types, is almost sure to follow in the practice of some workers, even if not generally or formally adopted. At the least, the names of higher categories with genera as types would become linked indirectly to the priority system because priority does apply, by rule, to generic names.

In consideration of (1) and (2) a sharp distinction must be made between categories now generally or frequently given names with uniform endings and those of which this is not true. Most authorities do now try to use uniform endings for groups from subtribe up to superfamily, but there are conflicting systems. Superfamilies are now almost always given the endings *-oidea* (especially for vertebrates) or *-acea* (especially for invertebrates). In occasional but now rare use are also *-ida, -oidae,* and perhaps a few others. There are also several systems for tribes and subtribes, but *-ini* and *-ina,* respectively, are now probably the most often used. Professor Stenzel proposes to use *-icae* for superfamilies and *-ici, -idi,* and *-ini* for supertribes, tribes, and subtribes, respectively.

Use of uniform endings for these particular categories seems highly desirable if only because the intention is now general, although the systems used are not. A choice should be made among the various systems and one, alone, endorsed. This should, I think, be based on usage and should follow the consensus in the absence of conclusive contrary arguments. In the meantime, it merely compounds confusion to propose use of still different endings (as *-icae* instead of either *-oidea* or *-acea* for superfamilies) or different uses of endings already current (as *-ina* for suborders instead of subtribes).

Beyond this point, it is my personal opinion that application of (1), uniform endings, to names of categories above superfamilies is undesirable as a general practice or rule. Rejection of the general principle would not necessarily exclude such application in limited groups as may seem suitable to specialists in those groups (as established for orders in Aves, for example [1]). Even in limited cases, however, the tendency to bring in (3) and (4), types and priority, seems to me deplorable and a real danger to be guarded against.

General application of uniform endings to, say, orders would at once wipe out or render almost unrecognizable a great majority of time-honored ordinal names in wide or universal use in the literature up to now. This is the more undesirable at such levels because names of high categories are more widely familiar and are used in less specialized literature than those of families or lower groups. It is hard to see how comprehension or stability would be promoted if, for instance, the Carnivora were called "Canida" or perhaps "Carnivorida," the Primates "Hominida" or "Primida," or the Coleoptera perhaps "Scarabaeida." Then, too, the change would invite a race by priority seekers for an infamous sort of immortality, and rival claimants would probably appear for many groups now enjoying complete stability of name. No one calls the Primates anything else now,

[1] There are special reasons why use of uniform endings based on generic names, and even application of the type concept to orders, is less objectionable in Aves than in most classes. Among these reasons is the structural uniformity of the birds. The whole class is little more varied in anatomy than some orders of mammals, and its orders are on the whole considerably less varied than many mammalian families. At the usual family level, the type system does have some justification.

but the scramble might be expected to produce (in the name of "uniformity") a dozen claimants such as "Primida," "Lemurida," "Pongida," "Hominida," etc., even if rival endings did not also compete. A decision to let priority decide would take us at once all the way to (4) of the sequence, with all that implies. I am one perhaps not of a majority but certainly of a large group of systematists who think priority (as opposed to usage) is definitely an evil. It may be a necessary evil at the level of genera and species, where in any event we seem to be stuck with it, but still it is an evil not to be extended unnecessarily.

The pedagogical and mnemonic arguments carry considerable weight but still seem to me insufficient to warrant a change for higher categories, even as regards (1) alone. If uniform endings, (1), were adopted but, in order to change names as little as possible, compounding from generic names, (2) were not, we would have such ordinal names as "Primatida" to learn and remember in place of Primates. Little could be gained on these grounds, and perhaps something would be lost. The only possible gain would be that a student, having learned that *-ida* means an order, would know at once that the "Primatida" are an order. It should still be most seriously weighed whether cashiering "Primates" is compensated by sparing a student the mental activity of associating the two words "Order Primates." In fact, would the student be spared? Are the concepts "order" and "Primates" really harder to learn under these symbols than under the symbols of "-ida" and "Primat-"? We might even consider how essential it is that the average student remember that the Primates are ranked as an order, or whether this really matters in comparison with the harder but essential task of learning what the Primates are like, where they live, and so on.

If, as would usually happen, adoption of proposal (1) also led to adoption of (2), the student would, indeed, be spared learning one root word for the concept Primates and another for the concept of, say, Hominidae, but this apparent gain is also decidedly questionable. The two concepts are, in fact, so different that the difference is absolutely essential in teaching or learning. Is it pedagogically or psychologically sound to invite, almost to enforce, confusion by calling one concept "Hominida" and the other "Hominidae"? (In passing, think of misprints, too!) Is it conducive to better understanding (or even to easier rote learning) to say "The lemurs are Hominida" instead of "The lemurs are Primates"? And think of the poor student, in whose name such proposals are usually made, who might be asked to remember some such system as this: The Hominica include shrews, bats, and armadillos but the Hominicae include none of these forms and also exclude the lemurs; Hominida include lemurs, monkeys, apes, and men but Hominidae are men, only; lemurs are not Hominina although monkeys and apes are, but monkeys and apes are not Homininae—and so on! I think most students would prefer to have clearly distinctive names for the most importantly different concepts and would settle for learning Unguiculata ("Hominica"), Primates ("Hominida"), and Anthropoidea ("Hominina"), before getting down to Hominoidea ("Hominicae"), Hominidae (*sic*), and Homininae (*sic*).

There is, finally, the fact that widespread introduction of names different from those already in the literature would mean that anyone, student or researcher, who used any literature published up to now would have to learn both the old names and the new, and also remember the relationship between the two sets. We would in fact more than double the task by an attempt to simplify it.

This would be no argument against a change that would finally justify itself, if only in the remote future, but it is a conclusive reason for making absolutely sure that the justification will eventuate.

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Some Nomenclatural Effects of the Paris Congress

The enactments of the 13th International Congress of Zoology at Paris in 1948 have required certain changes in the nomenclature of California fishes. The nature of the changes and the percentage of names to be changed, in respect to the total names in the fauna, are shown here.

Emendation of patronymics ending in *-ii*	2.6 %
Elimination of hyphens from compound names	1.5 %
Method of determining gender of generic names	1.4 %
Elimination of author's separates in determination of publication dates	0.6 %
Substitution of page precedence for rule of first reviser	0.6 %
Correction of misspelled patronymics	0.15%
Emendation of patronymics lacking a terminal *-i*	0.15%
Total changes	7.0 %

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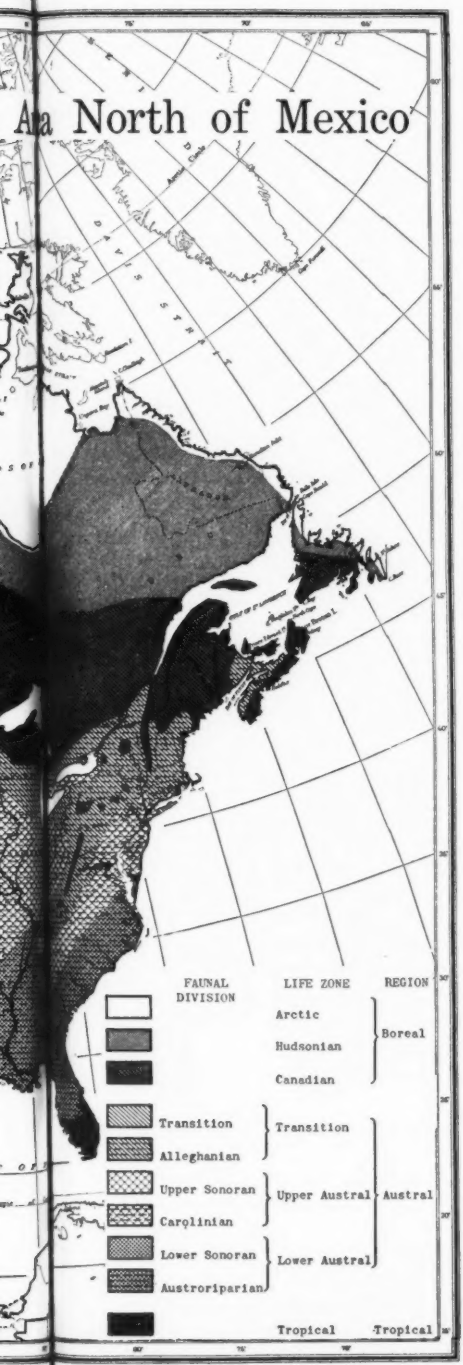
Life Zones of
ARCTIC OCEAN
GULF OF CALIFORNIA



Life Zone Map

The adjoining map of life zones of America north of Mexico was presented originally as the frontispiece of the recently published "Hymenoptera of America North of Mexico—Synoptic Catalog" by C. F. W. Muesebeck, Karl V. Krombein, Henry Townes, and others (U. S. Department of Agriculture, Agriculture Monograph No. 2, 1420 pp., 1951).

In many groups of Hymenoptera sufficient data have been analyzed so that the range of many species may be given succinctly by reference to life zones. Several contributors to the Synoptic Catalog reported specific distribution by life zones where possible, so that inclusion of a life zone map in the catalog was considered essential. It is reprinted here in the belief that it will be useful to other zoologists as well.

The map is an adaptation of the U. S. Biological Survey's Fourth Provisional Zone Map of North America, 1910, by C. Hart Merriam, Vernon Bailey, E. W. Nelson, and E. A. Preble. The only changes, other than those occasioned by the use of symbols instead of colors for the various zones, have been the addition of a small area of Tropical Zone at the extreme southern tip of Texas and a rezoning of Alaska on advice received from R. I. Sailer. It should be emphasized that with so great a reduction as used in the reproduction of this map, it has been necessary to represent narrow extensions and certain small isolated areas of some of the zones as more extensive than they actually are.

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The Nature of Systematics

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Part I.—The Classification of the Zoological Sciences

The uniting of more than a thousand persons in a society of systematic zoologists, for the purpose of advancing the science of systematic zoology, raises most pointedly the question of just what systematics is. In a field as large as all of zoology, there are a multitude of branches that have received special names and the attentions of certain specialists. Somewhere among this multitude is the science or group of sciences known as systematic zoology. If it really is a distinct field of endeavor, we should be able to define it and record its limits and its goals.

There is nothing about the names of the zoological sciences that shows their relationships to one another, as do colloid chemistry, organic chemistry, and physical chemistry. One zoologist may be a coleopterist (dealing only with beetles), another may be a nomenclaturist (dealing only with the procedures for naming animals), and still another a zoogeographer (dealing only with distribution, its patterns, and its causes). Specialists sometimes lose sight of the purpose and goals of their special fields and forget the interrelations of the related sciences, simply because their attention is not drawn to the matter. This sometimes results in false perspectives and in failure to carry out specialized work to the point where it will be useful in other specialties as well. A logical grouping or classification of the zoological sciences and in particular of the systematic fields may help to give us a proper perspective, as well as to show the scope and the importance of systematics in zoology.

First of all we must define three terms that we will need to use, and which are sometimes used in several meanings. The words taxonomy, systematics, and classification are defined by the dictionaries in almost the same terms. Yet there are three concepts which might well be distinguished if these or other terms can be restricted to them. Thus, classification can be taken to be the grouping of species and higher categories—the actual building of classified systems; systematics is the entire field dealing with kinds of animals, their distinction, classification, and evolution; and taxonomy can reasonably be restricted to the descriptive, discriminatory, and nomenclatural phase dealing mostly with species and lower categories, the data accumulated about them, and their names.

There is another word that requires clarification. The term morphology has been much used in place of anatomy or comparative anatomy. As recently pointed out by Snodgrass, morphology applies only to our mental concepts derived from evidence of anatomy, usually incapable of proof, but trying to explain the observed facts of anatomy. All our studies of the structure (internal or external) of animals are anatomy or comparative anatomy, until we begin to explain the facts or homologize the parts. Anatomical embryology (as distinct from physiological embryology) is part of anatomy, as are histology and cytology when comparative. Serology may be considered to be anatomy at the molecular level of organization, because the specificity of serological

reactions depends on molecular structure, but it seems more logical to treat it as part of comparative physiology.

In the broad sense the zoological sciences include all sciences dealing with animals. For example, our systematic fields, ecology, genetics, distribution, paleozoology, embryology, morphology, evolution, medicine, physiology, anatomy, histology, cytology, psychology, anthropology, and so on. The first step in classifying these is to determine whether they fall naturally into groups which can be defined on the basis of important criteria.

It appears to us that such a subdivision is obvious, as follows: Part of these subjects deal primarily with individuals of certain species, with the aim of aiding that individual or its species, as all the medical sciences deal with man. A second part of the zoological sciences deals with kinds of animals as distinguished from each other, as in comparative anatomy and systematics. A third part of the zoological sciences deals with the interactions of kinds of animals with their environment, as in ecology. Although hard and fast lines cannot be drawn in all cases, there does seem to be a real distinction between fields of interest in zoology on this basis—one or a few kinds for the direct or indirect benefit of man, the distinction between kinds, and the interaction of kinds (with other kinds or with the rest of the environment). Let us try grouping the various fields of zoology under these headings.

A. *Dealing with one or a very few kinds*

Here we have all the subjects dealing with the species *Homo sapiens* [medicine, psychology, physical anthropology], the subjects dealing with domestic and a few other animals [veterinary medicine, apiculture, animal breeding], and some general subjects with a predominantly medical aspect [physiological embryology, histology, cytology]. Some of these fields assume a position distinct from zoology, as medicine, psychology, anthropology, and veterinary medicine, but they are not basically distinct, since they deal with animals.

B. *Dealing with kinds as such*

There are several sciences that deal principally with determining the differences and similarities between kinds [comparative anatomy, comparative physiology, anatomical embryology, comparative histology and cytology, zoogeography, geological distribution], others that assemble the data into order and make them available for use [taxonomy, classification], still others that draw conclusions or generalizations of broad significance from the classified data [phylogeny, evolution], and finally those that investigate the causes of the observed facts and thus contribute to their recognition [morphology, stratigraphy, genetics].

C. *Dealing with kinds in relation to their environment*

Here we find only one field, known under various names [ecology, bionomics, natural history]. It would not be entirely illogical to include here also the social sciences, which deal largely with man in his environment, and the agricultural sciences, but they may reasonably be considered under A, where they are not part of zoology in the usual sense.

It is clear that our first category includes subjects that are generally not considered part of zoology, because they deal with man and a few species of animals used by man. In the third category ecology is a small and relatively new field which deals very extensively with botany as well as with zoology. It is quite distinct from both category one and category two, but it is very heavily dependent upon the taxonomic sciences.

The second category includes most of the subjects generally included in "zool-

ogy," and it is here that we must look for the field of systematics. These subjects all have one feature in common, they deal largely with *kinds*, defining, distinguishing, and arranging them, showing their separate ontogenies and phylogenies, and outlining the mechanisms that make kinds different and keep them so.

A review of these subjects will show that some (a) are concerned primarily with discovering and recording the data about kinds, their characteristics (both cytological, anatomical, embryological, and physiological) and their distribution. Others (b) are concerned primarily with the analysis of these data—by evaluating the data and classifying the kinds. And still others (c) draw conclusions from the data as classified, providing generalizations and theories to explain the observed data.

These three categories of zoological sciences are closely interlinked. When a taxonomist describes a new species, he observes and records data (a) and analyzes their systematic value—as being specific (b). When an evolutionist studies the subspecies of series of species and draws conclusions on the mechanism of subspeciation and speciation, he may be recording data (a), analyzing their systematic value (b), and generalizing from them (c) as to mechanisms that he cannot directly observe. It is thus not practicable to separate these three sub-categories clearly, and we are led to conclude that they are basically but one category —dealing with kinds as such.

There were in our original list, however, three subjects that seem to be somewhat different from the others. Their primary purpose is not to assemble or analyze data, nor to synthesize generalizations, although they may do one or more of these. It is rather to provide a background for understanding the data and mechanisms of certain of the other subjects. These three are morphology, stratigraphy, and genetics. Morphology is the science of form, dealing not with structures as such (as in anatomy) but with the origins, variations, homologies, and relations of structures. Stratigraphy, as it concerns paleontology (rather than mineralogy or structural geology), deals with the historical and geological aspects of fossils, explaining their occurrence, their fossilization, and their distribution. Genetics, while accumulating much data of its own, is finally concerned chiefly with explaining the mechanisms of heredity and evolution.

A common goal of explaining the data and mechanisms of other sciences is thus seen to unite these three. Their inclusion in the broad field of systematics is not so clearly required as is the case with the other sciences discussed, yet their primary function of contributing to systematics marks them as inseparable from it.

(1) Recording data

Comparative anatomy — Morphology
Comparative physiology
Anatomical embryology
Comparative histology
Comparative cytology
Zoogeography
Paleozoology — Stratigraphy

(2) Analysis of the data

Taxonomy
Classification

(3) Synthesis of generalizations

Phylogeny
Evolution — Genetics

The complete interdependence of these four groups of sciences indicates that they cannot be considered to be separate fields of zoology. If we cannot find a real division among the many sciences listed, we must conclude that they are all part of one class. This is strengthened by reflection on the purposes and aims

of each. Comparative anatomy, comparative physiology, and comparative embryology provide characters for discrimination of kinds; the knowledge of kinds and their classifications makes possible the recording of the data and also studies on relationships, phylogeny, and evolution; and these in turn provide explanations for the data, logical bases for the classifications, and generalizations about life and its development. The conclusion seems inescapable that these subjects are all part of one large field whose goals are the accumulation of data, the analysis and classification of the data, and the discovery of the underlying principles and mechanisms that produced the observed facts. This is what we have earlier defined as the broad field of systematics —the entire field dealing with kinds of organisms, their distinctive natures, their relationships, their phylogeny, and the mechanisms that have determined the nature, relationships, and phylogeny.

Part II.—The Aims and Tasks of Systematic Zoology

Systematics draws upon all sources of data about organisms for information in regard to their natures, and it must therefore concern itself directly or indirectly with the validity of all methods, principles, and practices which are used in characterizing these organisms and arranging them in accordance with their resemblances and differences. Those branches of zoology which contribute to systematics the facts of comparative anatomy, comparative physiology, and ecology have rather obvious and well understood aims and tasks. Their goals are to make known all the facts about the structures and functions and daily living of animals and to develop effective and suitable procedures for the study and reporting of such facts. But those branches of zoology and systematics which are primarily concerned with the *systematization* of this knowledge, i.e., with the characterization and grouping of *kinds* of organisms and the choice of biologically significant characters for doing this, require further treatment.

It is a strange fact that neither the nature of systematics nor its real aims and tasks are correctly understood in these times. For this situation the responsibility must be placed primarily upon the systematists themselves who have, for nearly a century, consistently misoriented and belittled the scope of their own work. Whereas, prior to the publication of Darwin's *Origin of Species*, the work of systematists was viewed as collecting, describing, naming, comparing, and grouping organisms, it has since been more commonly viewed as having one and only one major aim, which is implied in the statement that "the goal of taxonomy is the expression of phylogenetic relationships." At first this too-close identification of the tasks of the systematist with those of the student of evolution was revealed in the creation of numerous all-embracing phylogenies from Monera to man, or as the modern phrase puts it, "from Ameba to man," and phylogenetic speculation was rampant. It was an inevitable result of many decades of ancestor-hunting of this kind that inferences about ancestry came to be considered more important than the facts of comparative anatomy, as is clearly witnessed in the still prevalent definitions of homology which state that homologies are resemblances "due to common ancestry" and omit all other specifications as to the kinds and amounts of such correspondences. A moment's serious thought should make clear that inferences regarding ancestry must be derived in the first instance from the amounts and kinds of structural or biochemical correspondence, the latter referring to structure on the molecular level of the

proteins characteristic of the body, and that to define homology as "resemblance based on common ancestry" is to misunderstand the nature and limitations of the available knowledge.

This misorientation of systematics is revealed also in recent writings characteristic of "The New Systematics." Thus Huxley (1940) has stated that "Fundamentally, the problem of systematics regarded as a branch of general biology is that of detecting evolution at work." Here the scope of systematics has apparently been reduced to the narrowest possible dimensions—that of "detecting," which presumably means witnessing, the actual process of evolution in the present, or perhaps analyzing its immediate results. Nothing is said in this statement about the grand tasks of collecting, describing, naming, comparing, and grouping the results of one thousand million years of evolution. Actually, "The New Systematics" is broader than Huxley's quotation would imply, and it seems evident from the rest of his writings that he intended no such limitation on the scope of systematics. But, on the other hand, much of the recent writing in the field of the new systematics does seem to accord with the specification of "detecting evolution at work." For instance we may turn to the recent definitions of species to be found in Dobzhansky (1937) and Mayr (1942). In Dobzhansky's *Genetics and the Origin of Species* we find the proposal to define species "as that stage of evolutionary process at which the once actually and potentially interbreeding array of forms becomes segregated in two or more separate arrays which are physiologically incapable of interbreeding." And we find also a similar concept in Mayr's definition, "Species are groups of actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups." In these writings it is evident that a close fusion of the detail of evolution in the present with the business of systematics has been attempted and that the great task of "arranging the myriad forms of life" (Simpson, 1945) has been inadequately represented.

We quote from Simpson in regard to the nature of taxonomy.

"The science of arranging the myriad forms of life is taxonomy. . . . A formal classification of animals is a part of taxonomy, but only a part when 'taxonomy' is used in its full sense, and it is also a practical application of some particular set of taxonomic principles, of which many different sets are possible and useful." (Taxonomy in the "full sense" as defined by Simpson is equivalent to our systematics. Confusion due to the use of one term in both broad and narrow senses can easily be avoided by our usage.)

"Taxonomy [that is, systematics], is at the same time the most elementary and the most inclusive part of zoology, most elementary because animals cannot be discussed or treated in a scientific way until some taxonomy has been achieved, and most inclusive because taxonomy in its various guises and branches eventually gathers together, utilizes, summarizes and implements everything that is known about animals, whether morphological, physiological, psychological, or ecological."

"Classification consists of grouping things according to their characteristics or properties, placing them in a system of categories, and applying a designation to each group thus established."

It is obvious that Simpson's statements are greatly different from Huxley's characterization of the business of systematics as "detecting evolution at work," though these two authorities probably agree much more in regard to the nature of systematics than the quotations given indicate. However that may be, the relation between certain of the subdivisions of systematics, such as taxonomy, phylogeny, and classification must be clarified. In the first place the relations

between taxonomy and classification are intimate and inseparable, so much so that it is convenient to refer to a taxonomist as one engaged in the description, naming, and arranging of organisms. But a taxonomist may or may not be also a phylogenist, for the nature of the relation between taxonomy and phylogeny is of a less immediate kind. Let us speak of the taxonomist as the person engaged in describing and grouping organisms and of the phylogenist as the person who attempts to trace their ancestry. If taxonomy is to be considered scientific in its methods, the bulk of the knowledge concerned with it must be observable and verifiable. But not only must the facts of animal organization used by the taxonomist be "hard facts," but the grouping of organisms based on such facts must be a logical process.

Now the grand object of classification everywhere is the same. It is to group the objects of study in accordance with their essential natures. Any scheme of grouping based on other attributes which results in placing together unlike objects and separating like objects will defeat the major purpose of grouping. But in biology, since Darwin's *Origin of Species* appeared, biologists have substituted for this grand object that of "expressing the phylogenetic relationships" of organisms, a substitution which has introduced endless confusion into taxonomic theory and practice. The complex relations between taxonomy and phylogeny have been discussed by many authors, including Huxley (1940) and Simpson (1945). Let us attempt to clarify the relations between them by listing the aims and tasks of those who work in each field.

The aims and tasks of the taxonomist

1. To participate directly or indirectly in the great task of collecting representative samples of existing and preexisting organisms.

2. To describe these organisms in all their essential characters as accurately and as fully as possible.

3. To compare these organisms objectively and quantitatively.

4. To develop a set of principles in regard to the choice and relative importance of those characters which will permit a grouping of the organisms possessing them in accord with their esential natures.

5. To group organisms on the basis of the characters chosen into a series of more and more inclusive categories, and to give appropriate names to the categories needed.

6. To develop keys and other devices for the recognition and identification of organisms and to help non-systematists learn the names and relationships of the organisms with which they deal.

7. To help clarify the place of systematics in general biology and to give some thought to the improvement of the training of general biologists and professional taxonomists.

For comparison with these aims and tasks of the taxonomist we list those of the phylogenist, who seeks to trace the broad outlines of racial descent. This is also a complex and difficult task, a necessary motif for the taxonomist according to Parker but not so considered by us. The challenge of bringing order out of chaos, of properly arranging "the myriad forms of life" in accordance with their natures, is quite a sufficient motivating force.

The aims and tasks of the phylogenist

1. To study the operation of genetic mechanisms in order to describe and understand the nature of the evolutionary processes in the present.

2. To analyze the genetic mechanisms responsible for homologous correspondences and thus to determine the ways in which somatic character expressions may indicate common genetic mechanisms.

3. To distinguish among traits of all kinds those which are conditioned mainly by heredity.

4. To determine which characters are primitive and which are specialized and to determine also whether the characters have diverged, run parallel, or converged.

5. To distinguish clearly fact from theory in phylogeny and to attempt to determine whether present knowledge in regard to heredity and variation can provide an evolutionary mechanism which could, if sufficiently extended, account for the production of all the types of animals which now or formerly inhabited the earth.

This listing of the aims and tasks of the taxonomist and the phylogenist separately should help to clarify the relation between them, a relationship which has also been effectively discussed by Huxley (1940) and Simpson (1945). "Phylogeny cannot be observed." This quotation from Simpson is characteristically frank and indicates that conclusions relating to phylogeny are based on inferences from observed facts regarding the observable characters of organisms and their space-time distribution. On the other hand the characters of existing organisms can be known and the characters of preexisting organisms can be known in part, depending upon the extent of the preservation of the ancestral characteristics. Phylogeny can at best be but fragmentary as to the characters of any organisms and as to the proportion of organisms having a sufficiently adequate fossil representation to make a phylogeny possible. On the other hand taxonomy must be universal. Conservative estimates indicate that there has been at least a thousand million years of evolution on the earth, and of this period less than half is documented with fossil remains, and in a very spotty and fragmentary manner at that. Furthermore, this documentation is not only a fragmentary one for most animal groups, but the later stages rather than the early formative stages are the ones which are preserved and which represent most commonly the hard parts only. Many groups of soft-bodied animals are practically unrepresented in the record and their origin cannot be known, though the present nature of their existing representatives can become known in whole and that of their preexisting representatives may be presumed.

To continue to subscribe to the view that "the goal of taxonomy is the expression of phylogenetic relationships" or even that a classification must be based on phylogeny is not now and never has been justified. We grant that the classification of the results of evolution may have "a phylogenetic background," and that in general the present nature and phylogenetic methods will, if correctly applied, yield concordant results. But the systematic method based on the present natures of the organisms grouped must be given the precedence over the phylogenetic method (1) because of the universality of the former and (2) because of the greater objectivity and verifiability of the facts used in the present nature method. Furthermore, the only reason why a phylogenetic method may be useful in systematics is that, where the evidences permit, it will give results in accord with the present or essential nature method. If the phylogenetic method, which groups according to presumed ancestry, should give results greatly unlike the present nature method, the former would be a useless method for the real and necessary purposes of systematics. However important the phylogenetic method may be for the student of evolution, therefore, it must be given second place by the taxonomist, for his great aim must be to group existing organisms on the basis of their natures and by means of the characters which show what the organisms are *now*, and to group preexisting organisms on the basis of what they *were* as indicated by their fossils, and not to group any organisms on the basis of what their ancestors were or may have been.

Part III.—Perspectives in Systematics

It is time that universal taxonomy claimed its birthright. To collect, describe, name, compare, and group organisms by sound methods and principles is a great undertaking far from completion.

As progress in taxonomy is made we shall then know better and more completely what kinds of organisms there are and were. From the systematized knowledge of animal kinds we may come to understand what evolution has accomplished; for it must be obvious to all that we cannot understand the nature of the process evolution without knowing what kinds of organisms it has produced. It is the work of the taxonomist which provides the facts about the species and higher categories of animals from which are drawn inferences in regard to their phylogeny and the nature of the evolutionary mechanisms responsible. Without the work of the taxonomist neither the evolutionist nor the phylogenist nor anyone else would know what they were talking about or have anything significant to say.

A new era in systematics lies before us. This era cannot be referred to as relating only to "The New Systematics" of the past decade. It will, we hope, not be entirely either *New Systematics* or *Old Systematics* but SYSTEMATICS. In a manner analogous to the action of natural selection, the good characters of the Old and the New systematics will be selected and integrated into a functioning and harmonious organism. This SYSTEMATICS will attempt to classify the results of a thousand million years of evolution and to develop adequate principles for so doing. It must be frank about the limitations of available knowledge, and, while ever seeking to extend the limits of this knowledge, it will not wittingly confuse the facts of animal organization with theories and inferences in regard to their remote evolutionary history. In this spirit, a new generation of taxonomists may emerge, trained to be as rigidly scientific in methods as geneticists or biochemists, and to classify existing organisms as they are, or preexisting organisms as the record indicates they were, but in neither case on the basis of inadequately documented inferences regarding their assumed ancestors. The accepted general guiding principles will be as universal as taxonomy itself, applicable to all kinds of animals, whether known only from the present, or whether, at the other extreme, belonging to a fossil lineage that can be determined with high probability. And above all, the grand object of classification will be realized, which is that organisms will be grouped in accordance with their natures so that the human mind may deal effectively with "the myriad forms of life."

REFERENCES

Dobzhansky, Th. 1937. Genetics and the Origin of Species. Columbia University Press, New York.

Huxley, J. 1940. The New Systematics. Oxford Press.

Mayr, E. 1942. Systematics and the Origin of Species. Columbia University Press, New York.

Simpson, G. G. 1945. The Principles of Classification and a Classification of Mammals. *Bull. American Mus. Nat. Hist.*, vol. 85.

DR. RICHARD E. BLACKWELDER, Associate Curator, Division of Insects, U. S. National Museum, Washington, D. C.

PROF. ALAN BOYDEN, Professor of Zoology and Director of The Serological Museum at Rutgers University, New Brunswick, N. J.

DISCUSSION

The Law of Priority

Recent attacks on the Law of Priority as the basis of our nomenclature prompted the Nomenclature Committee to try to find out the views of American workers on this subject. The following statement and ballot were sent to about 800 members.

"It appears that the Law of Priority, which many taxonomists have regarded as the cornerstone of nomenclature, is now under strong attack in some quarters (cf. Hemming, 1950, Minutes of the Meeting of the ICZN, 1948, *in* Bull. Zool. Nomenclature, vol. 4, p. 233. 'No member of the Section spoke in favour of the continuance of the present system under which the Law of Priority not only failed to promote uniformity but was itself actively instrumental in leading to confusion and instability in nomenclature').

"Believing that at least some of the American members of the International Commission on Zoological Nomenclature may not know or may not correctly reflect the general viewpoint of American taxonomists, and may thereby give the Commission and taxonomists in other countries a wrong impression, this sampling of opinion is undertaken to try to determine the predominant viewpoint. If no one reaction predominates, the results may still show the direction of thinking by the majority.

"Although it is realized that there may be as many variants in philosophy as there are taxonomists, an endeavor has been made to draft brief statements that will approximate the beliefs of various groups.

"Please check (X) the statements on the accompanying ballot sheet which you believe comes closest to expressing your own viewpoint. Space is provided for any additional comments which you wish to make.

1. ☐ I favor the STRICT application of priority, with no exceptions.
2. ☐ I favor REASONABLY STRICT application of priority, with few exceptions, that must be virtually universally approved (e.g.: *Musca domestica, Plasmodium,* etc.), where confusion would result in other fields as well.
3. ☐ I favor MODERATE APPLICATION of priority, with more exceptions than in No. 2, but applications for these exceptions must be carefully and fully documented and must receive general approval. Generally, exceptions should be made only for cases of real confusion, i.e., not for straight synonymy but to prevent REVERSAL OF USAGE in important names.
4. ☐ I favor PRIORITY as a general principle, in theory, but with reasonably free exceptions, to be made in any group whenever the specialists in that group so agree, regardless of how important the case is and whether confusion or mere synonymy is involved.
5. ☐ I favor USAGE as the guiding principle, regardless of priority."

The voting on these choices was as follows:

1. 33 (Strict application of priority)
2. 119 (Reasonably strict application of priority)

3. 187 (Moderate application of priority)
4. 29 (Free exceptions)
5. 3 (Usage as the guiding principle)

From these returns it appears that 91% of the taxonomists replying favor priority as a general principle and believe that exceptions to it should be at least limited to those fully and carefully documented and receiving general approval.

THE NOMENCLATURE COMMITTEE, SSZ

The Copyright of ICZN Opinions

A survey of the attitude of American taxonomists toward the practice of the International Commission on Zoological Nomenclature of copyrighting its publications has been completed by the Nomenclature Committee of the SSZ. The results are reported below. The survey consisted of the following statement and ballot of four choices.

"Since 1943 the publications of the ICZN (Opinions and Declarations rendered by the ICZN and the Bulletin of Zoological Nomenclature) have borne the statement 'All Rights Reserved.'

"The secretary of the Commission has stated that this was done to protect the financial interest of the Commission in the excess stock of these publications.

"There are some who feel that this material is of public nature and should receive the widest possible dissemination. Although the copyright is not in force in the U.S. (as of April, 1950), and perhaps these publications could not be copyrighted here because of their public nature, the result is much the same because publishing companies feel obligated to respect copyrights of other countries. Therefore, because the secretary of the Commission did not authorize quotations, at least two important works designed for the training of students in taxonomy have had to paraphrase all references to ICZN opinions published since 1943.

"Please check the statement which comes closest to expressing your viewpoint. Comments may be made on the reverse.

1. ☐ I favor the strict application of the copyright in order to safeguard the financial interests of the International Trust for Zoological Nomenclature.
2. ☐ I favor the copyright but believe that the Trust should publicly state its permission to make short quotations of portions of opinions, etc., in connection with discussions of nomenclatural matters in scientific papers.
3. ☐ I favor the copyright to protect the Trust against unauthorized and unprofessional reprinting and believe that the Trust should publicly state that it permits quotations such as those indicated in 2 above, AND that, on application, any bona fide zoologist or zoological organization will be given permission to reproduce material such as rules, amendments, opinions, or parts thereof in text or reference works on taxonomy, nomenclature, and related matter.
4. ☐ I favor complete elimination of the copyright."

The voting on these choices was as follows:

1. 1 (Strict application)
2. 9 (Partial quotation permitted)
3. 218 (Professional quotation permitted on application)
4. 106 (Complete elimination of copyright)

It is clear that 97% of the persons replying, or 324 taxonomists out of 334, believe that the copyright should not be used to interfere with reprinting of rules or opinions in reference works or should be eliminated entirely.

THE NOMENCLATURE COMMITTEE, SSZ

IN THE MUSEUMS

Introducing the Royal Ontario Museum of Zoology and Palaeontology

The Royal Ontario Museum as a whole, constituted by an Act of the Ontario Legislature in 1912, now consists of a family of three autonomous museums, in the fields of archaeology, geology—mineralogy, and zoology—palaeontology. The Royal Ontario Museum of Zoology and the Royal Ontario Museum of Palaeontology, formed in 1913, were amalgamated in 1951.

For thirty-five years this family of museums received support via two channels: Half came through the University of Toronto, which is essentially a provincial institution, and half directly from the Ontario government to the museum's Trustees. In 1948 the museums were placed entirely under the university.

Though provincial in origin and maintenance, the scope of the Royal Ontario Museum of Zoology and Palaeontology has no restricting article of purpose. The original and prevailing terms were calculated to include the world as its field of interest. Consequently several of its collections are already widely representative. This will be evident in the brief descriptive digest which follows.

During the early stages of the museum's development, its small staff was primarily and necessarily concerned with activities enforced by the rapidly growing collections and in establishing the institution as an educational force in a metropolitan community. The idea of a museum in Ontario's capital city, Toronto, was over-ripe by the time of its realization, thus physical congestion was experienced within a very few years after the first building was opened in 1914. Although building accommodation was tripled in 1932, the institution again finds itself healthily crowded in 1952. This is true of both its research ranges and rooms as well as its galleries.

The museum's research collections have been acquired from miscellaneous private and institutional sources; a lesser amount from planned expeditions and purchases. A policy from the beginning has permitted curatorial discretion with respect to acceptance of bequests so that virtually no useless material usurps useful space. We commend this policy to new museums.

Although wisely the scope of the museum was unlimited, it has been equally wisely recognized that the first obligation and real opportunity lie at its very door. Studies made on the animals of this province need not be provincial in concept. Since Ontario's boundaries embrace an area extending approximately 1,000 miles east-west and north-south, and since few extant animals from this area were represented in any collections prior to this museum's birth, the immediate task was obvious. Furthermore, the geological history of this region, with particular reference to glaciation and the Great Lakes system, determines that studies on Ontario's fauna have particular zoogeographic significance. For example, they may well aid in suggesting refugia and differentiation areas not generally appreciated. By widening the horizon for

investigation to include the vast Canadian field, rich fossil beds are added in the west and east and little known or unknown boreal hinterlands await attention.

Although a single Canadian province might be considered an adequate laboratory, we have not lost sight of the need for comparative material from foreign faunas. Through purchase and exchange and incidental collecting beyond our boundaries, the museum's world collections have shown gratifying growth and not infrequently we have been able to take our place among the world family of museums in assisting the researches of other institutions through the loan of comparative material pertinent to studies on far off faunas. Admittedly no compartment of the museum is beyond the youthful stage of development and some appendages are still embryonic. Perhaps some estimate of the status of the several collections and nature of current activities will reveal the stage of development.

Beginning with fossil invertebrates which constitute a division of the museum, there is a history both with respect to collections and studies which antedates the present institution, but which merges naturally with it along with personalities and collections. As with most Divisions, it represents an outgrowth or evolution, not a spontaneous creation. Studies such as that of the late Dr. W. A. Parks (one-time Director of the Royal Ontario Museum of Palaeontology) on Devonian stromatoporoids of North America were based extensively on material in this museum. This is also true of the continuous contributions of the present Curator of Fossil Invertebrates (and Associate Director) Dr. Madeline A. Fritz, an authority on Bryozoa, and the studies of the late Dr. W. S. Dyer. The catalogue of the invertebrate fossil collection contains nearly 27,000 entries, lots and specimens. This includes more than 15,000 species and an estimated total of 60,000 specimens. Among the rich type series included, there are more than 300 holotypes.

The fossil vertebrate division is particularly noteworthy for its collection of reptiles, especially dinosaurs. Most of these were acquired through the museum's own expeditions to the bad lands of Canada under the supervision of Mr. L. Sternberg (Associate Curator). Of the several type specimens in the museum, ten are duck-billed dinosaurs described by Dr. Parks. Four types of bird-like dinosaurs also described by Parks are especially noteworthy. Concerning vertebrate fossils other than reptiles, there is a substantial nucleus of fishes and mammals. As is the rule in museums of palaeontology, birds form a minor part.

With reference to living forms, the museum has a considerable collection of invertebrates representing several distinct classes which are safely housed but dormant insofar as specialist use is concerned. As with most museums such material presents a real problem for curatorial judgment.

The museum's Division of Entomology (which includes Arachnology) is actively though inadequately staffed. Mr. T. B. Kurata (Assistant Curator) has brought together a collection of spiders numbering approximately 55,000 specimens, representing about 500 species. This collection, largely Canadian in origin, is being identified and organized. More than a dozen species new to science have been discovered, three of which have been formally described. The insect collection totals approximately 300,000 specimens. It contains the important Walker collection of Odonata, on which Dr. E. M. Walker's revisions of the genera *Aeschna* and *Somatochlora* were based. At present Dr. Walker (Honorary Curator of Entomology) is engaged in a revision of the Zygoptera of Canada. Other elements of the insect collection have been studied by various workers. More particularly, Orthoptera has received attention by Dr. Walker and by Dr. F. A. Urquhart (Curator of Entomology and Director of the Royal Ontario Museum of Zoology and Palaeontology); Plecoptera by Dr. Wm.

E. Ricker and Dr. Urquhart; Sarcophagidae, Dolichopodidae, and Culicidae by Dr. Urquhart and others; Tipulidae by Dr. J. Speed Rogers; and Tabanidae by Dr. L. L. Pechuman. A total of 53 holotypes, species and subspecies, is contained in the collections.

The importance of fisheries to Canada and the emphasis on the study of fishes which is traditional at the University account for the rapid initial growth of the museum's fish collection. Professor J. R. Dymond (formerly Director of the Royal Ontario Museum of Zoology) devoted attention to this collection for some years, but more recently its founding as a division under the curatorship of Dr. W. B. Scott has furthered its organization and program. Dr. Scott's personal interest in Coregonidae and Salmonidae has brought about a revision of the collection of these groups. The total fish collection numbers some 16,000 lots and specimens, representing approximately 650 species. This division also includes amphibians and reptiles, groups not extensively represented in Canadian faunas, but Mr. E. B. S. Logier (Associate Curator) has brought together over 7,700 lots and specimens and is currently undertaking an orderly review of the collection.

The bird collection was the first to be organized as a division of the museum under the curatorship of the writer with the aid of Mr. J. L. Baillie (Research Assistant). The largest single acquisition of the division was the collection of the late J. H. Fleming (Honorary Curator f Ornithology) received by bequest in 1940. The Fleming collection comprised some 32,000 specimens, rich in exotics including island and vanishing forms. The particular program of the division has been concerned with Ontario's neglected avifauna. Twenty-four expeditions have yielded collections which are rich in breeding, juvenal, and natal down plumages. With the growth of comparative material several generic revisions and racial studies have been undertaken, and the distributional limits of the bird fauna have been refined considerably. With the thought of future investigations respecting the relationship of larger taxonomic groups, attention is being devoted to osteological specimens which now consist of 367 skeletons representing 240 species. The total bird collection, not including nests and eggs, consists of approximately 80,000 specimens of world birds representing all bird families and an estimated 80% of the genera. Nineteen types, of species or subspecies, are included.

In the early stages of the museum's development, prior to the budding off of the several divisions, the building of a mammal collection received considerable attention, particularly during the course of Ontario surveys. This provided a nucleus of several thousand specimens for the newly-established division under the curatorship of Dr. R. L. Peterson and Mr. S. C. Downing (Research Assistant). Currently studies concerned with eastern Canadian forms are under way and studies on *Alces* and *Lynx* have been completed. Recent activity has brought about marked growth in the skeletal collection and in the representation of fur-bearers. The total collection of specimens approximates 20,000, representing somewhere in the neighbourhood of 700 species.

The foregoing digest indicates something of the size, activity, and stage of development of the Royal Ontario Museum of Zoology and Palaeontology. Although this museum is at present the largest of its kind in Canada, it is in point of years extremely youthful. Its staff appreciates that many problems and probably mistakes have been avoided by consultation with, and some knowledge of, considerably older and much respected museums in other places.

L. L. SNYDER, Associate Director

SSZ NEWS

The Annual Meeting, December 1951

This year the Annual Business Meeting preceded the Council meeting, on December 28th. It was held in the Benjamin Franklin Hotel in Philadelphia immediately after the Annual Breakfast. Both these functions were unusually well attended, with 39 members taking part.

The absence of President Hubbs because of illness was announced. He sent greetings to the society and profound regrets at not being able to attend. The society expressed the hope that his recovery would be rapid and complete. In his place the meeting was presided over by President-Elect Alfred S. Romer.

The report of the Secretary may be summarized as follows: The membership has reached 1030, of which 116 are from 26 countries outside of the U. S. In the recent election the following officers of the society for 1952 were elected: President-Elect, Prof. H. B. Hungerford; Secretary-Treasurer, Dr. R. E. Blackwelder; and Councillors, Prof. D. F. Hoffmeister and Prof. H. W. Manter. The balloting on the proposed amendments to the constitution resulted in accepting the proposals by a vote of 449 to 40; therefore we now have a single class of membership instead of the previous Active and Associate, we will accept no life members, the terms of office of the officers end at the annual meeting, and subscription to any publications shall be optional to members.

The Treasurer read the summary of the finances of the society approved by the Auditing Committee, as of November 30, 1951, the end of the fiscal year. The society received from all sources $2466.64 and spent for all purposes $929.36, leaving a balance of $1537.28, which was the bank balance on that date. Of these amounts, $962.14 (consisting of balance from 1950, current dues and arrears, and gifts) was available for general expenses during 1951, and $827.81 was spent on postage, stationery, duplicating, News Letters, express on the book exhibit, office equipment, and a membership drive. The Publication Fund received gifts of $1437.50, and expenditures from it were $88.50.

The Nomenclature Committee reported the results of its ballots on Priority and Copyright, as shown on a previous page in this issue. They are considering other problems of nomenclature and expect to circulate another ballot to determine the views of members.

The Education Committee reported progress on the preparation of a book on the fundamentals of systematics and plans to continue also its investigation of the problems of instruction in systematics by sending its previous questionnaire to the many members who have joined since it was circulated in 1949.

The Abstracts and Bibliographies Committee reported the completion of an arrangement for the society to act as agent for the *Zoological Record* among our members, as announced in *News Letter No. 5*. It was also stated that discussions have been under way to determine the extent to which it is desirable for *Biological Abstracts* to deal with purely systematic papers.

The plans for the new journal were summarized, with the results seen in this first issue. The selection of a managing editor was put at the head of the agenda

for the Council for 1952, and it is hoped that the selection can be announced in the next issue. In the meantime the Secretary will act as Editor pro tem, and a temporary Editorial Board has been appointed to assist him. Because time is short for arranging the first few issues, most of the members of this board are in Washington, where they can be consulted with least delay.

Material is solicited for future issues, especially short discussion articles, news of individuals, new courses, expeditions, deposit of collections, meetings, etc., summaries of the systematic programs of museums and other institutions, descriptions of the organization and programs of societies interested in systematics, discussions of new books, and any other matter that would be of interest to systematists.

In future issues the SSZ News section will be reprinted and will be sent to members who do not subscribe to *Systematic Zoology*. In this way all members will continue to receive news of the society's activities.

A report on the actions of the Council at its December 1951 meeting will be published in the next issue.

The 1952 Annual Meeting will be in St. Louis December 27-30, 1952. Plans already well developed show that this will be an outstanding meeting. There is opportunity for a more integrated and smooth-running meeting, and every attempt will be made to capitalize on this opportunity. Efforts will also be made to take limited part in the Corvallis, Oregon, meeting of the Pacific Section of the AAAS in June, in the Ithaca meeting of the AIBS in September, and in the Philadelphia meeting of the Entomological Society of America at Philadelphia early in December.

People and Projects

Continuing its program of making taxonomic surveys of the fishes of the rivers of Texas, the Department of Wildlife Management of the Texas Agricultural and Mining College recently completed a survey of the Brazos River system. One of the collections made near Seymour, Texas, in this survey proved to be of special interest since it contained more than 200 topotypes of *Cyprinodon bovinus rubrofluviatilis*. This fish is rare in collections and has a doubtful status, so the acquisition of a large number of specimens will do much to clarify the situation. Other rivers already surveyed in this program are the Red River and its tributaries between Oklahoma and Texas and the Guadalupe River system.

The National Science Foundation has announced that it will support basic research in the biological sciences by making grants for such research to educational or other institutions or to individuals. Ordinarily grants will be awarded to institutions for research by specified individuals. In reviewing proposals the Foundation will emphasize the scientific merit of the suggested research, including the competence of the scientist under whom the study will be made.

F. Stearns MacNeil, of the U. S. Geological Survey, is studying the little-known atolls of the Northern Marshalls. Information will be sought on the geology and the marine biology and ecology of the reefs and lagoons, as well as on the life of the islands. Mr. MacNeil's specialties are Tertiary paleontology and stratigraphy.

C. H. Lowe, Jr., recently transferred to the Department of Zoology at the University of Arizona, is continuing his work on general ecology and the systematics of amphibians and reptiles.

David G. Frey, now Associate Professor of Zoology at Indiana University, is director of the Indiana Lake and Stream Survey and of the Winona Lake Biological Station. He offers graduate course work in limnology and opportunities for graduate work in various phases of limnology.

Dr. John D. Goodman of the University of Redlands (Calif.) recently completed a two-summer taxonomic investigation of the Cercariae of Reelfoot Lake, Tenn. His doctoral thesis at Ann Arbor was entitled, "Taxonomy and distribution of digenetic trematodes of the family Ochetosomatidae."

Cornell University is offering a lecture-seminar course in zoological nomenclature. It is to include discussion of the history and nature of zoological nomenclature and of attempts that have been made to regulate it; the work of the International Commission of Zoological Nomenclature; and the present rules governing zoological nomenclature in the light of the revisions made at Paris in 1948. The first term was conducted by **Dr. J. Chester Bradley.** Six professors and 36 graduate students enrolled, coming from the fields of biology, zoology, ornithology, ichthyology, oceanography, conservation, entomology, limnology, veterinary medicine, and paleontology.

The Mexican government has recently organized a Commission for the Development of Rural Fishculture (C.F.P.R.). This commission is working with both cold and warm water species as well as with oyster culture. A small number of papers have been published thus far. The C.F.P.R. is endeavoring to compile a list of persons interested in related fields of fish culture and fisheries biology. Persons desiring to receive publications from this office should forward their names and addresses to Fernando Obregon F., Jefe de la Comisión para el Fomento de la Piscicultura Rural, Mexico 7, D.F., Mexico.

[A few news notes submitted last year for the News Letter did not get published. Some of these now seem to be a little out of date and should be resubmitted.

In order to make the journal more attractive to you personally, we solicit notes on persons, projects, courses, expeditions, special publications, and anything of interest to systematists.

Longer notices or short articles on the taxonomic programs of museums or societies are also invited. News of such organizations, descriptions of their work, and staff projects are especially welcome.]

Current Addresses Needed

Mail has been returned unclaimed from the last known address of the following members:

Donald G. Allen
Ray Allison
Willie C. Johnson
George E. Klak
Lloyd A. Le Zotte
Everett Lyle Schiller
L. Vargas

The Secretary would greatly appreciate receiving the current address of any of these from anyone who can furnish it.

Changes of Address

The following changes of address (or corrections) have been recorded since November 1, 1951.

Beal, Dr. R. S., Jr. 1951. Conservative Baptist Seminary, 1500 East 10th Ave., Denver 3, Colo. (Coleoptera: Anthicidae, Dermestidae)

Blake, Mrs. D. H. 1950. 3416 No. Glebe Road, Arlington 7, Va. (Chrysomelidae)

Burkenroad, Martin D. 1948, C. Institute of Marine Science, Port Aransas, Tex. (Penaeidae)

Carpenter, Dr. Esther. 1948, C. 55 Prospect Str., Northampton, Mass. (Amphibia, Oligochaeta)

DeMeules, Mr. Donald. 1950. 6031 W. River Drive, Minneapolis 12, Minn.

Dowling, Dr. Herndon G. 1950. Haverford College, Haverford, Pa. (Reptiles, amphibians)

Emerson, William K. 1949. Museum of Paleontology, Univ. of California, Berkeley 4, Calif. (Mollusca)

Evenden, Fred G., Jr. 1948. 3434 57th Str., Sacramento 20, Calif. (Aves, Reptilia, Amphibia)

Farrell, Charles E. 1948. Box 25, Vanderbilt Univ., Nashville 4, Tenn. (Chiggers)

Flury, Alvin G. 1950. Box 552, Mathis, Tex. (Reptilia, Amphibia)

Gillaspy, James E. 1949. 313 Hosack Str., Taylor, Tex. (Bembicidae)

Gillogly, Lorin Roy. 1950. 5804 21st Ave., Sacramento 20, Calif. (Coleoptera: Nitidulidae)

Gould, Douglas J. 1950. Army Medical Service Graduate School, Army Medical Center, Washington 12, D. C. (Acarina)

Gressitt, Dr. J. Linsley. 1950. Pacific Science Board, c/o B. P. Bishop Museum, Honolulu 17, Hawaii. (Coleoptera, myriapods, reptiles, amphibians, birds)

Hall, Mr. Gordon E. 1951. 615 E. Grand, McAlester, Okla. (Fishes)

Harry, Harold W. 1948, C. Zoology Dept., Univ. of Missouri, Columbia, Mo. (Mollusca)

Hooijer, Dr. D. A. 1950. Rijksmuseum Nat. Hist., Leiden, Netherlands. (Recent & fossil mammals)

Klauber, L. M. 1948, C. 233 West Juniper Str., San Diego 1, Calif. (Rattlesnakes)

Knight, Dr. Kenneth L. 1948, C. Malaria & Mosquito Control Unit No. 1, Naval Air Station, Jacksonville, Fla. (Mosquitoes)

Kring, James B. 1951. Conn. Agric. Exp. Sta., P.O. Box 1106, New Haven 14, Conn. (Aphididae, Symphyta)

Leonard, Dr. Justin W. 1950. Michigan Dept. of Conservation, Lansing 13, Mich. (Aquatic insects)

McElvare, Rowland R. 1951. 26 Bogart Ave., Port Washington, N. Y. (Lepidoptera: Phalaenidae)

Netting, Dr. M. Graham. 1948, C. Carnegie Museum, Pittsburgh 13, Pa. (Herpetology)

Offutt, Dr. Edward P. 1948, C. 4515 Roxbury Drive, Bethesda 14, Md. (Parasitology)

Paynter, Raymond A., Jr. 1949. Kent Drive, Hamden, Conn. (Birds)

Prychodko, Dr. W. 1950. 4811 John R Street, Detroit 1, Mich. (Mammals)

Quate, Larry W. 1950. Dept. of Entomology, Univ. of Nebraska, Lincoln 1, Neb. (Diptera)

Rabb, Robert L. 1949. Div. of Biological Science, N. C. State College, Raleigh, N. C. (Tingidae)

Rivero, Juan A. 1948, C. 356A Harvard Street, Cambridge, Mass. (Herpetology)

Roback, Selwyn S. 1950. Dept. of Limnology, Acad. Nat. Sci. Philadelphia, Philadelphia 3, Pa. (Sarcophagidae)

Robinson, John H. 1951. Route 8, Box 535, Waco, Tex. (Coleoptera: Cicindelidae, Carabidae)

Russell, Harold G., Jr. 1950. U. S. Public Health Service, 2222 W. Weldon Ave., Phoenix, Ariz. (Siphonaptera, Diptera)

Scott, Dr. Donald C. 1949. U. S. Public Health Service, Box 2038, Hill Sta., Augusta, Ga. (Fishes, aquatic insects)

Scott, Harold George. 1950. 814 West Roma Ave., Albuquerque, N. Mex. (Collembola)

Skinner, Mr. Frank E. 1951. P.O. Box 8, Albany 6, Calif. (Hymenoptera: Chalcidoidea)

Sturtevant, Dr. Frank M., Jr. 1950. Div. of Biological Research, P. O. Box 5110, Chicago 80, Ill. (Drosophilidae)

Thurlow, Mrs. John. 1948, C. National Research Council, 2101 Constitution Ave., Washington 25, D. C. (Crustacea)

Vannucci, Dr. M. 1951. Alameda Franca 1608, São Paulo, Brasil. (Coelenterata, Cestoda, Oligochaeta)

Supplemental List of Members

This list is a first supplement to the complete list of members issued in December 1951.

Alverson, Dayton Lee. 1952. 17916 Brittany Drive, Seattle 66, Wash. [Marine fisheries, marine birds]

Bentinck, William C. 1952. Dept. of Entomology, Univ. of California, Berkeley 4, Calif.

Boyle, W. Wayne. 1952. Dept. of Entomology, Cornell Univ., Ithaca, N. Y. [Coleoptera: Erotylidae]

Briggs, John Carmen. 1952. Natural History Museum, Stanford Univ., Calif.

Brooks, Dr. John L. 1952. Osborn Zoological Lab., Yale Univ., New Haven, Conn. [Cladocera]

Cary, Mrs. C. Reed. 1952. Ellet Lane & Wissahickon, Mt. Airy, Philadelphia 19, Pa. [Sphingidae]

Cliff, Frank Samuel. 1952. Natural History Museum, Stanford Univ., Calif.

Cook, Carl. 1952. Crailhope, Ky. [Odonata, Papilionidae]

Dewailly, Dr. Vet. P. 1952. 94 Avenue de Suffren, Paris (XV), France. [Coleoptera: Melolonthidae]

Dunn, Margaret E. 1952. School of Fisheries, Univ. of Washington, Seattle 5, Wash. [Echinoderida]

Farrier, Maurice H. 1952. Box 5215, State College Station, Raleigh, N. C. [Acari]

Fehlmann, Herman Adair. 1952. Natural History Museum, Stanford Univ., Calif.

Finks, Robert M. 1952. 85-32 253rd Street, Bellerose 6, N. Y. [Tetracoralla, fossil Porifera]

Forster, Dr. E. W. 1952. Menzingerstrasse 67, München 38, Germany. [Lepidoptera]

Garner, William V. 1952. 2140 Ward Street, Berkeley 5, Calif.

Haltenorth, Dr. Th. 1952. Zoologische Staatssammlung, Menzingerstrasse 67, München 38, Germany. [Mammals]

Howden, Henry F. 1952. Box 5215, State College Station, Raleigh, N. C. [Coleoptera]

Hull, Robert W. 1952. Zoology Dept., Univ. of Illinois, Urbana, Ill. [Protozoa, invertebrates]

Jones, Prof. E. Ruffin, Jr. 1952. Dept. of Biology, Univ. of Florida, Gainesville, Fla. [Turbellaria, Archiannelida]

Knapp, S. E. 1952. Dept. of Zoology, Univ. of Idaho, Moscow, Idaho. [Copepoda, Turbellaria]

Knight, Prof. Harry H. 1952. Dept. of Zoology, Iowa State College, Ames, Iowa. [Hemiptera: Miridae]

Lantz, John Perry. 1952. Univ. of Miami, Miami 46, Fla. [Herpetology, endocrinology]

Lee, Robert D. 1952. 181 Loma Linda Drive, Loma Linda, Calif.

Levine, Dr. Norman D. 1952. College of Veterinary Medicine, Univ. of Illinois, Urbana, Ill. [Parasitic Protozoa, Nematoda, Cestoda, Trematoda, Arthropoda]

Leviton, Alan Edward. 1952. Natural History Museum, Stanford Univ., Calif.

Markell, Dr. Edward K. 1952. Dept. of Infectious Diseases, UCLA Medical School, Los Angeles 24, Calif. [Trematoda: Gorgoderidae; Protozoa: Coccidia]

Mead, Giles Willis. 1952. Natural History Museum, Stanford Univ., Calif.

Milstead, Wm. W. 1952. Dept. of Zoology, Univ. of Texas, Austin 12, Tex. [Fishes, amphibians, reptiles, mammals]

Newcombe, Dr. Curtis L. 1952. 8 Middle Road, Hidden Valley, Lafayette, Calif. [Crustacea, Mollusca]

Patten, Dr. John A. 1952. Dept. of Biology, Middle Tennessee State College, Murfreesboro, Tenn. [Parasitology, ornithology, embryology]

Quay, W. B. 1952. Museum of Zoology, Univ. of Michigan, Ann Arbor, Mich. [Mammalia, Anoplura]

Russell, Dr. F. E. 1952. Dept. of Biology, Calif. Inst. of Technology, Pasadena, Calif. [Fishes]

Sadlick, Walter. 1952. Geology Dept., Univ. of Utah, Salt Lake City 1, Utah. [Brachiopoda]

Schroeder, William C. 1952. Museum of Comp. Zoology, Cambridge 38, Mass. [Fishes]

Sohn, Mr. I. G. 1952. U. S. Geological Survey, Washington 25, D. C. [Ostracoda]

Stockton, William D. 1952. 107 Dryden Road, Ithaca, N. Y. [Invertebrates]

Storey, Margaret Hamilton. 1952. Natural History Museum, Stanford Univ., Calif.

Taylor, Dr. Walter P. 1952. 424 West Harrison Street, Claremont, Calif. [Mammals]

Van Gelder, Mr. Richard G. 1952. Museum of Natural History, Univ. of Illinois, Urbana, Ill. [Mammals]

Vockeroth, J. R. 1952. Systematic Entomology, Science Service, Ottawa, Ont., Canada. [Diptera]

Weisenhorn, Don J. 1952. Box 190, St. Ambrose College, Davenport, Iowa.

Welander, Dr. Arthur D. 1952. Fisheries Center, Univ. of Washington, Seattle 5, Wash. [Fishes]

Yount, J. L. 1952. Zoology Dept., Univ. of Hawaii, Honolulu 10, Hawaii. [Pelagic tunicates]

Necrology

Sherman Chauncey Bishop, who joined the Society in 1950, died on May 28, 1951, at the age of 63. He was Professor of Vertebrate Zoology at the University of Rochester and was interested in spiders, fishes, reptiles and amphibians.

Alfreda Berkeley Needler, a charter member of the Society, died during November 1951, at the age of 48. Dr. Needler was a volunteer investigator for the Fisheries Biology Board of Canada and was interested in the taxonomy of pycnogonids and decapod crustaceans.

William Frederick Clapp, a charter member of the Society, died on December 28, 1951. He was president and director of the William F. Clapp Laboratories, Inc. and was interested in marine invertebrates.

We record also the deaths of ten zoologists who were not members of our Society, but who contributed to the field of systematic zoology. **Guy C. Crampton,** October 31, 1951; morphology and evolution of arthropods. **Emlen P. Darlington,** May 14, 1951; Lepidoptera. **William Procter,** April 19, 1951; amateur entomologist. **T. Wayland Vaughan,** January 16, 1952; geologist, specialist on corals and larger Foraminifera. **Lawrence Paul Wehrle,** October 23, 1951; entomologist. **Edward A. Birge,** June 9, 1950; limnology. **Edmund Fleutiaux,** November 25, 1951; Coleoptera. **Leonard C. Sanford,** December 7, 1950; ornithology.

Walter R. Sweadner, January 13, 1951; entomology. **Dudley Moulton,** July 5, 1951; Thysanoptera.

It is also appropriate for systematic zoologists to take note of the death of the leading authority on the flowering plants of eastern North America, whose monumental revision of Gray's Manual of Botany will be used by many zoologists. **Merritt Lyndon Fernald** was 77 at the time of his death on September 22, 1950. He had been associated with Harvard University for nearly 60 years.

Additional Books Exhibited at Philadelphia

Most of the books exhibited at Cleveland, as listed in News Letter No. 4, were again exhibited at Los Angeles and at Philadelphia. In addition, the following were received during 1951 and were added to the exhibit. The arrangement corresponds with that used previously.

Invertebrates

Borradaile, L. A. et al. The Invertebrata, 725 pp., Cambridge, 1935.

Hegner, R. W. Invertebrate zoology, 570 pp., Macmillan, 1933.

Hyman, L. H. The invertebrates, Platyhelminthes and Rhynchocoela, 550 pp., McGraw-Hill, 1951. [Vol. II.]

Hyman, L. H. The invertebrates, Acanthocephala, Aschelminthes and Entoprocta, 572 pp., McGraw-Hill, 1951. [Vol. III.]

Protozoa

Kirby, H. Materials and methods in the study of protozoa, 72 pp., California, 1950.

Parasitology

Cable, R. M. An illustrated laboratory manual of parasitology, 152 pp., Burgess, 1950.

Goody, T. Soil and freshwater nematodes, 390 pp., Wiley, 1951.

LaPage, G. Parasitic animals, 351 pp., Cambridge, 1951.

Riley, W. A. Introduction to the study of animal parasites and parasitism, 87 pp., Burgess, 1942.

Mollusks

Morris, P. A. A field guide to the shells, 236 pp., Houghton Mifflin, 1951.

Rogers, J. E. The shell book, 508 pp., Branford, 1936.

Smith, M. East coast marine shells, 314 pp., (author), 1937.

Smith, M. A review of the Volutidae, 127 pp., (author), 1942.

Myriapods

Wang, Y. M. The Myriapoda of the Philippine Islands. *Serica,* vol. 1, pp. 1-78.

Arachnida

Gertsch, W. J. American spiders, 285 pp., Van Nostrand, 1949. [The New Illustrated Naturalist series.]

Insects

Arnett, R. H., Jr. A revision of the nearctic Oedemeridae (Coleoptera). *American Midl. Nat.,* vol. 45, pp. 257-391. 1951.

Brues, C. T. Insect dietary, 466 pp., Harvard, 1946.

Brues, C. T. Insects and human welfare, 154 pp., Harvard, 1947.

Essig, E. O. College entomology, 900 pp., Macmillan, 1942.

Gradwohl, R. B. H. (editor). Clinical tropical medicine, 1647 pp., Mosby, 1951.

Hagan, H. R. Embryology of the viviparous insects, 472 pp., Ronald, 1951.

Herms, W. B., Medical entomology, 643 pp., Macmillan, 1950.

Holland, W. J. The butterfly book, 424 pp., Doubleday, 1931.

Imms, A. D. Insect natural history, 317 pp., Blakiston, 1951.

Jaques, H. E. How to know the beetles, 372 pp., Brown, 1951.

Klots, A. B. A field guide to the butterflies, 349 pp., Houghton Mifflin, 1951.

Metcalf, R. L. Destructive and useful insects, 1071 pp., McGraw-Hill, 1951.

Muesebeck, C. F. W. & Krombein, K. V. Hymenoptera of America north of Mexico. Synoptic catalog. *U. S. Dept. of Agriculture Monograph* No. 2, 1420 pp., 1951.

Park, O. Cavernicolous pselaphid beetles of Alabama and Tennessee, with observations on the taxonomy of the family. *Geological Survey of Alabama, Museum Paper* No. 31, 107 pp.

Robertson-Miller, E. Butterfly and moth book, 285 pp., Scribner's, 1931.

Stokoe, W. J. The observer's book of British butterflies, 191 pp., Frederick Warne.

Wigglesworth, V. B. Insect physiology, 134 pp., Wiley, 1950.

Vertebrates

Newman, H. H. The phylum Chordata, 477 pp., Macmillan, 1939.

Fishes

Alvarez, J. Claves para la determinacion de especies en los peces de las aguas continentales mexicanas. Secretaria de Marina, Mexico, D. F. 1951.

Carlander, K. D. Handbook of freshwater fishery biology, 281 pp., Brown, 1950.

Hornell, J. Fishing in many waters, 210 pp., Cambridge, 1950.

Nichols, J. T. Representative North American fresh-water fishes, 128 pp., Macmillan, 1942.

Amphibians and reptiles

Ditmars, R. L. A field book of North American snakes, 305 pp., Doubleday, 1949.

Ditmars, R. L. The reptiles of North America, 476 pp., Doubleday, 1936.

Ditmars, R. L. Reptiles of the world, 321 pp., Macmillan, 1933.

Ditmars, R. L. Snakes of the world, 207 pp., Macmillan, 1931.

Holmes, S. J. The biology of the frog, 386 pp., Macmillan, 1934.

Ingle, R. M. & Smith, F. G. W. Sea turtles and the turtle industry, 107 pp., Univ. of Miami, 1949.

Birds

Benson, S. V. The observer's book of British birds, 224 pp., Frederick Warne.

Bent, A. C. Life histories of North American wild fowl, vol. 1, 244 pp.; vol. 2, 316 pp., Dover, 1951.

Bond, J. Field guide of birds of the West Indies, 257 pp., Macmillan, 1947.

Davison, V. E. Bobwhites on the rise, 150 pp., Scribner's, 1949.

Delacour, J. Birds of Malaysia, 382 pp., Macmillan, 1947.

Delacour, J. & Mayr, E. Birds of the Philippines, 309 pp., Macmillan, 1946.

Friedmann, H. The parasitic cuckoos of Africa. *Washington Acad. Sci. Monograph* No. 1, 204 pp., 1948.

Griscom, L. & Folger, E. V. The birds of Nantucket, 156 pp., Harvard, 1948.

Jaques, H. E. Field note books for studying birds, 119 pp., Brown, 1947.

Mayr, E. Birds of the southwest Pacific, 316 pp., Macmillan, 1945.

Peterson, R. T. A field guide to western birds, 240 pp., Houghton Mifflin, 1941.

Pough, R. H. Audubon bird guide, 312 pp., Doubleday, 1949.

Pough, R. H. Audubon water bird guide, 352 pp., Doubleday, 1951.

Reed, C. A. Bird guide, 238 pp., Doubleday, 1951.

Saunders, A. A. A guide to bird songs, 307 pp., Doubleday, 1951.

Stoddard, H. The bobwhite quail, 559 pp., Scribner's, 1931.

Sutton, G. M. Mexican birds, 282 pp., Univ. of Oklahoma, 1951.

Mammals

Carter, T. D., Hill, J. E. & Tate, G. H. H. Mammals of the Pacific world, 227 pp., Macmillan, 1946.

Linsdale, J. M. & Tevis, L. P. The dusky-footed wood rat, 664 pp., Univ. of California, 1951.

Stokoe, W. J. The observer's book of British wild animals, 224 pp., Frederick Warne.

Paleontology

Smith, B. W. The world in the past, 365 pp., Frederick Warne, 1931.

Woods, H. Palaeontology invertebrate, 477 pp., Cambridge, 1946.

General Biology

Marsland, D. Principles of modern biology, 757 pp., Henry Holt, 1951.

Parker, J. B. & Clarke, J. J. An introduction to animal biology, 554 pp., Mosby, 1949.

Stauffer, A. Introductory biology, 722 pp., Van Nostrand, 1949.

Ville, C. A. Biology: The human approach, 580 pp., Saunders, 1950.

Winchester, A. M. Biology and its relation to mankind, 777 pp., Van Nostrand, 1949.

General Zoology

Ensminger, M. E. Animal Science, 1059 pp., Interstate, 1950.

Hall, T. S. A source book in animal biology, 716 pp., McGraw-Hill, 1951.

Hegner, R. W. & Stiles, K. A. College zoology, 911 pp., Macmillan, 1951.

Newman, H. H. Outlines of general zoology, 661 pp., Macmillan, 1936.

Storer, T. I. General zoology, 832 pp., McGraw-Hill, 1951.

Weimer, B. R. Man and the animal world, 569 pp., Wiley, 1951.

Winchester, A. M. Zoology, the science of animal life, 437 pp., Van Nostrand, 1947.

Comparative anatomy

McEwen, R. S. Vertebrate embryology, 699 pp., Henry Holt, 1949.

Prosser, C. L. (editor). Comparative animal physiology, 888 pp., Saunders, 1950.

Stromsten, F. A. Davison's Mammalian anatomy, 349 pp., Blakiston, 1947.

Taylor, W. T. & Weber, R. J. Functional mammalian anatomy (with special reference to the cat), 575 pp., Van Nostrand, 1951.

Natural history

Bates, N. B. East of the Andes and west of nowhere, 237 pp., Scribner's, 1947.

Ditmars, R. L. Confessions of a scientist, 241 pp., Macmillan, 1934.

Ditmars, R. Thrills of a naturalist's quest, 268 pp., Macmillan, 1932.

Graham, V. & Robertson, G. V. Strange sea life, 115 pp., Henry Holt, 1950.

Hornaday, W. T. The American natural history, 449 pp., Scribner's, 1935.

Humphreys, D. The zoo book, 39 pp., Henry Holt, 1947.

Pycraft, W. P. (editor). The standard natural history from amoeba to man, 942 pp., Frederick Warne.

Roughley, T. C. Wonders of the great barrier reef, 282 pp., Scribner's, 1947.

Russell, F. S. & Yonge, C. M. The seas, 379 pp., Frederick Warne, 1936.

Smith, F. G. W. Atlantic reef corals, 112 pp., Univ. of Miami, 1948.

Webb, C. Animals from everywhere, Frederick Warne, 1950.

Evolution

Blum, H. F. Time's arrow and evolution, 222 pp., Princeton, 1951.

Schmalhausen, I. I. Factors of evolution, 327 pp., Blakiston, 1949.

Genetics

Darlington, C. D. & Mather, K. The elements of genetics, 446 pp., Macmillan, 1949.

Dunn, L. C. Genetics in the 20th century, 634 pp., Macmillan, 1951.

Snyder, L. H. The principles of heredity, 515 pp., Heath, 1951.

Waddington, C. H. An introduction to modern genetics, 441 pp., Macmillan, 1939.

White, M. J. D. The chromosomes, 124 pp., Wiley, 1950.

Ecology

Hesse, R., Allee, W. C. & Schmidt, K. P. Ecological animal geography, 715 pp., Wiley, 1951.

Principles & Methods

Baker, J. R. Cytological technique, 211 pp., Wiley, 1950.

Bennett, A. H. et al. Phase microscopy, 320 pp., Wiley, 1951.

Boole, G. An investigation of the laws of thought, 424 pp., Dover, 1854. [Reprint.]

Gatenby, J. B. & Beams, H. W. (editors). Lee's The Microtomist's Vade-Mecum, 753 pp., Blakiston, 1950.

Gray, P. Handbook of basic microtechnique, 141 pp., Blakiston, 1952.

Welch, P. S. Limnological methods, 381 pp., Blakiston, 1948.

Whyte, L. L. The unitary principle in physics and biology, 162 pp., Henry Holt, 1949.

Conservation

Grange, W. B. The way to game abundance, 365 pp., Scribner's, 1949.

Leopold, A. Game management, 481 pp., Scribner's, 1933.

History

Essig, E. O. A history of entomology, 1029 pp., Macmillan, 1931.

Locy, W. A. Biology and its makers, 477 pp., Henry Holt, 1935.

Sears, P. B. Charles Darwin, the naturalist as a cultural force, 124 pp., Scribner's, 1950.

Literature

Zoological Record. Zoological Society of London.

Vol. 85, for 1948. (1951) Whole volume.

Sect. 1, Comprehensive zoology (Mar. 1951)
2, Protozoa (Oct. 1950)
4, Coelenterata (Apr. 1951)
6, Vermes (Mar. 1951)
7, Brachiopoda (Aug. 1950)
8, Bryozoa (Oct. 1950)
9, Mollusca, (May 1951)
10, Crustacea (June 1950)
11, Trilobita (Nov. 1950)
12, Arachnida (Dec. 1950)
15, Pisces (Apr. 1951)
16, Amphibia & Reptilia (Sep. 1950)
18, Mammalia (Sep. 1950)
19, List of new generic and subgeneric names recorded in volume 85 (May 1951)

Vol. 86, for 1949.

Sect. 1, Comprehensive zoology (Oct. 1951)
2, Protozoa (Sep. 1951)
3, Porifera (Sep. 1951)
5, Echinoderma (Aug. 1951)
8, Bryozoa (Oct. 1951)
10, Crustacea (Aug. 1951)
14, Protochordata (Sep. 1951)
15, Pisces (Oct. 1951)
16, Amphibia & Reptilia (Oct. 1951)
17, Aves (Sep. 1951)
18, Mammalia (Aug. 1951)

Journals

Bulletin of Marine Science of the Gulf and Caribbean, University of Miami Press, Coral Gables, Fla. Vol. 1, No. 1.

Piscicultura Rural, Secretaria de Marina, Mexico, D. F. Vol. 1, Nos. 1-11.

Publicaciones Cientificas, Servicio Oceanografico y de Pesca, Ministerio de Industrias y Trabajo, Montevideo, Uruguay. Nos. 3, 4, 5, 1950; No. 6, 1951.

Publications of the Institute of Marine Sciences, The University of Texas, Austin, Tex. Vol. 1, No. 1.

The Serological Museum, Bureau of Biological Research, Rutgers University, New Brunswick, N. J. Bull. No. 1-6.

The Wasmann Journal of Biology, The University of San Francisco, Calif. Vol. 8, No. 3, 1951; Vol. 9, No. 1, 2, 1951.

Books Listed by Publishers

The Blakiston Co. (1012 Walnut St., Philadelphia 5, Pa.)

Lee's The microtomist's vade-mecum, edited by J. B. Gatenby & H. W. Beams. (11th ed.) $8.50.

Handbook of basic microtechnique, by P. Gray.

Insect natural history, by A. D. Imms.

Factors of evolution, by I. I. Schmalhausen. $6.00.

Davison's Mammalian anatomy, by F. A. Stromsten. $4.50.

Limnological methods, by P. S. Welch. $7.00.

Wm. C. Brown Co. (915 Main St., Dubuque, Iowa)

Handbook of freshwater fishery biology, by K. D. Carlander. $4.50.

Field note books for studying birds, by H. E. Jaques, $1.00.

How to know the beetles, by H. E. Jaques. $4.25 (bound); $3.25 (spiral).

Charles T. Branford Co. (551 Boylston St., Boston 16, Mass.)

The shell book, by J. E. Rogers. $6.50.

Burgess Publishing Co. (426 So. 6th Str., Minneapolis 15, Minn.)

An illustrated laboratory manual of parasitology, by R. M. Cable. $2.50.

Introduction to the study of animal parasites and parasitism, by W. A. Riley. $1.75.

Cambridge University Press (51 Madison Ave., New York 10, N. Y.)

The Invertebrata, by L. A. Borradaile et al. $6.00.

Fishing in many waters, by J. Hornell. $6.00.

Parasitic animals, by G. LaPage. $4.00.

Palaeontology Invertebrate, by H. Woods. $2.75.

Doubleday & Co., Inc. (Garden City, N. Y.)

A field book of North American snakes, by R. L. Ditmars. $3.75.

The reptiles of North America, by R. L. Ditmars. $6.75.

The butterfly book, by W. J. Holland. $12.50.

Audubon bird guide, by R. H. Pough. $3.00.

Audubon water bird guide, by R. H. Pough. $3.50.

Bird guide, by C. A. Reed. $1.95.

A guide to bird songs, by A. A. Saunders. $3.50.

Dover Publications, Inc. (1780 Broadway, New York 19, N. Y.)

Life histories of North American wild fowl, vol. 1 and 2, by A. C. Bent. $8.00.

An investigation of the laws of thought, by G. Boole. $4.50.

Harvard University Press. (44 Francis Ave., Cambridge 38, Mass.)

Insect dietary, by C. T. Brues, $5.00.

Insects and human welfare, by C. T. Brues. $3.00.

The birds of Nantucket, by L. Griscom & E. V. Folger. $3.75.

D. C. Heath & Co. (285 Columbus Ave., Boston 16, Mass.)

The principles of heredity, by L. H. Snyder. $4.75.

Henry Holt & Co. (257 4th Ave., New York, N. Y.)

Strange sea life, by V. Graham & G. V. Robertson. $2.50.

The zoo book, by D. Humphreys. $2.50.

Biology and its makers, by W. A. Locy. $3.90.

Principles of modern biology, by D. Marsland. $5.25.

Vertebrate embryology, by R. S. McEwen. $4.90.

The unitary principle in physics and biology, by L. L. Whyte. $3.50.

Houghton Mifflin Co. (2 Park St., Boston 7, Mass.)

A field guide to the butterflies, by A. B. Klots. $3.75.

A field guide to the shells, by P. A. Morris. $3.75.

A field guide to western birds, by R. T. Peterson, $3.50.

The Interstate Printers & Publishers, Inc. (19 N. Jackson St., Danville, Ill.)

Animal science, by M. E. Ensminger. $6.00.

The Macmillan Co. (60 5th Ave., New York 11, N. Y.)

Field guide of birds of the West Indies, by J. Bond. $4.00.

Mammals of the Pacific world, by T. D. Carter, J. E. Hill, & G. H. H. Tate, $3.50.

The elements of genetics, by C. D. Darlington & K. Mather. $5.25.

Birds of Malaysia, by J. Delacour. $5.00.

Confessions of a scientist, by R. L. Ditmars. $4.00.

Reptiles of the world, by R. L. Ditmars. $3.95.

Thrills of a naturalist's quest, by R. L. Ditmars. $4.00.

Birds of the Philippines, by J. Delacour & E. Mayr. $4.00.

Snakes of the world, by R. L. Ditmars. $3.95.

Genetics in the 20th century, by L. C. Dunn. $5.00.
A history of entomology, by E. O. Essig. $14.00
College entomology, by E. O. Essig. $6.50.
College zoology, by R. W. Hegner & K. A. Stiles. $5.50.
Invertebrate zoology, by R. W. Hegner. $5.00.
Medical entomology, by W. B. Herms, $9.00.
The biology of the frog, by S. J. Holmes. $4.40.
Birds of the southwest Pacific, by E. Mayr. $4.00.
Outlines of general zoology, by H. H. Newman. $5.00.
The phylum Chordata, by H. H. Newman. $5.00.
Representative North American fresh-water fishes, by J. T. Nichols. $1.75.
An introduction to modern genetics, by C. H. Waddington. $3.75.

McGraw-Hill Book Co., Inc. (330 W. 42nd Str., New York 18, N. Y.)
A source book in animal biology, by T. S. Hall. $10.00.
The invertebrates, Platyhelminthes and Rhynchocoela, by L. H. Hyman. [Vol II.] $9.00.
The invertebrates, Acanthocephala, Aschelminthes and Entoprocta, by L. H. Hyman. [Vol. III.] $9.00.
Destructive and useful insects, by R. L. Metcalf. $5.50.
General zoology, by T. I. Storer. $6.00.

C. V. Mosby Co. (3523 Pine St., St. Louis, Mo.)
Clinical tropical medicine, edited by R. B. H. Gradwohl. $22.50.
An introduction to animal biology, by J. B. Parker & J. J. Clarke. $4.00.

Princeton University Press. (Princeton, N. J.)
Time's arrow and evolution, by H. F. Blum. $4.00.

The Ronald Press Company. (15 E. 26th Str., New York 10, N. Y.)
Embryology of the viviparous insects, by H. R. Hagan. $6.50.

W. B. Saunders Company. (218 W. Washington Squ., Philadelphia 5, Pa.)
Comparative animal physiology, edited by C. L. Prosser. $12.50.
Biology: The human approach, by C. A. Villee. $5.00.

Charles Scribner's Sons. (597 5th Ave., New York 17, N. Y.)
East of the Andes and west of nowhere, by N. B. Bates. $4.00.
Bobwhites on the rise, by V. E. Davison.
The way to game abundance, by W. B. Grange. $6.00.
The American natural history, by W. T. Hornaday. $7.50.
Game management, by A. Leopold. $7.50.
Butterfly and moth book, by E. Robertson-Miller. $3.50.
Wonders of the great barrier reef, by T. C. Roughley. $5.00.
Charles Darwin, the naturalist as a cultural force, by P. B. Sears. $2.50.
The bobwhite quail, by H. Stoddard. $10.00.

University of California Press. (Berkeley 4, Calif.)
Materials and methods in the study of Protozoa, by H. Kirby. $2.50.
The dusky-footed wood rat, by J. M. Linsdale & L. P. Tevis. $7.50.

University of Miami Press. (Coral Gables 46, Fla.)
Sea turtles and the turtle industry, by R. M. Ingle & F. G. W. Smith.
Atlantic reef corals, by F. G. W. Smith. $3.75.

University of Oklahoma Press. (Faculty Exchange, Norman, Okla.)
Mexican birds, by G. M. Sutton. $10.00.

D. Van Nostrand Co., Inc. (250 Fourth Ave., New York 10, N. Y.)
American spiders, by W. J. Gertsch. $6.95.
Introductory biology, by A. Stauffer. $5.50.
Functional mammalian anatomy, by W. T. Taylor & R. J. Weber. $7.25.
Biology and its relation to mankind, by A. M. Winchester. $5.75.
Zoology, the science of animal life, by A. M. Winchester. $5.00.

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Contributions of the following types are solicited: Papers on principles and the applications of principles of wide implication and general interest in any phase of systematics, such as comparative anatomy, zoogeography, paleontology, taxonomy, classification, evolution, or genetics; discussions of methods, specific problems, and activities of systematists; discussions of new books and monographs; and news of systematists, organizations interested in systematics, research and teaching programs, expeditions, collections, meetings, and anything else of interest to systematists.

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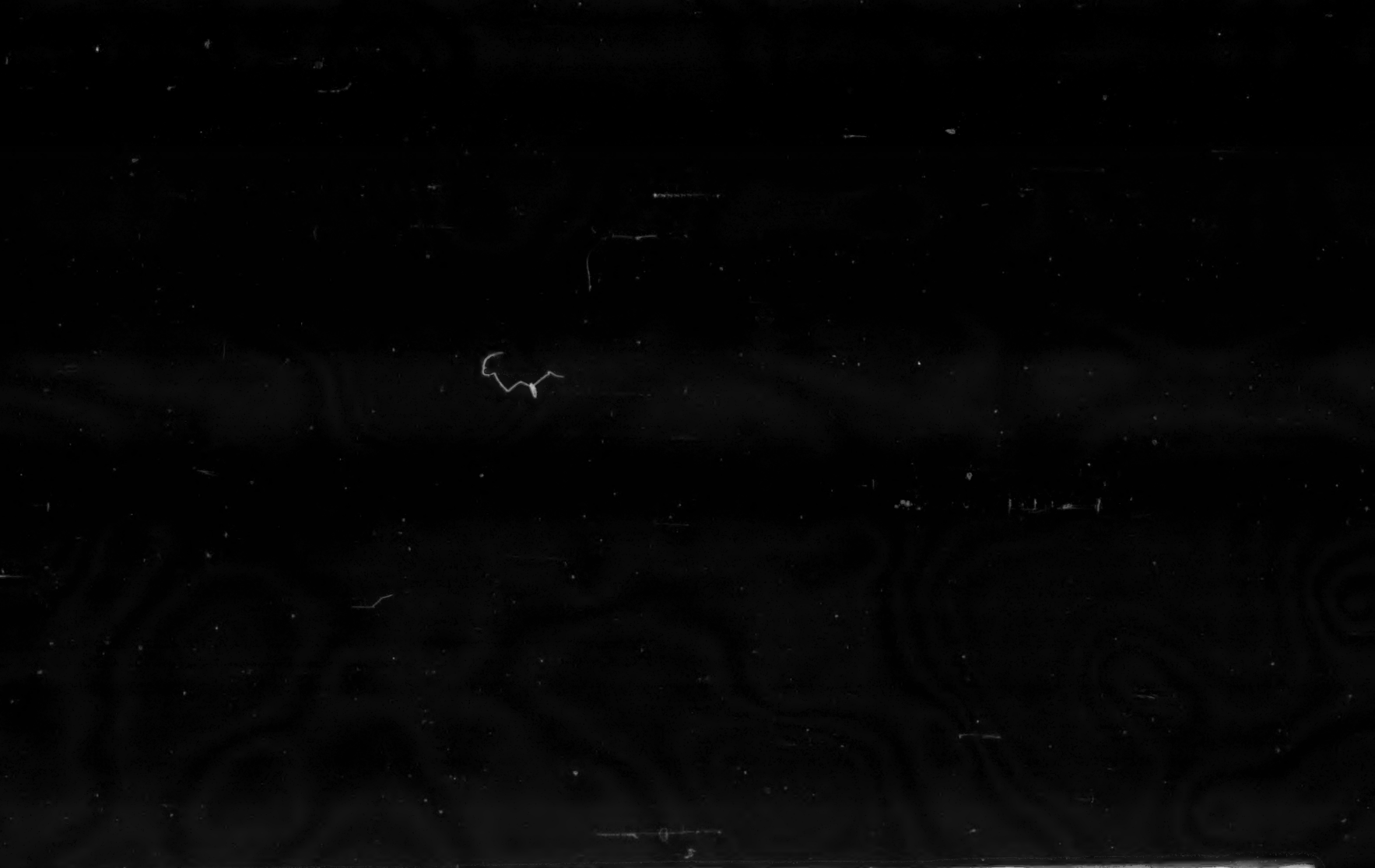
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